PROCEEDINGS

OF THE

ROYAL SOCIETY OF EDINBURGH.

VOL. IX. 1877–78. No. 102.

NINETY-FIFTH SESSION.

Monday, 15th April 1878.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. On Vortex Vibrations, and on Instability of Vortex Motions. By Sir William Thomson.

2. On the Theory of Vowel Sounds. By Professor M'Kendrick.

3. Preliminary Note on a Method of Detecting Fire-Damp in Coal Mines. By Professor George Forbes.

The author exhibited two instruments, both founded upon the same principles, for measuring the quantity of fire-damp present in a coal mine. The first instrument consists of a tuning-fork fixed above the open end of a resonating tube, whose other end is closed by a piston whose position (read off on a scale) regulates the length of the resonating tube. The length of the tube, which resounds to the definite pitch of the tuning-fork, depends upon the nature of the gas with which it is filled. The more fire-damp, the longer is the tube. Barometric pressure has no effect upon this instrument. The correction for temperature is made by reading off, not a fixed mark upon the piston, but the top of the mercury of a thermo

meter attached thereto, of dimensions determined by actual experiment. The only source of error to which the instrument seems liable is the counteracting influence of dense carbonic acid gas in choke-damp. But it is found that the presence of choke-damp destroys the explosive character of fire-damp; and, so far as experiments go, it seems certain that, in all cases when the presence of choke-damp prevents the instrument from indicating the presence of fire-damp, the fire-damp is denuded of its explosive character.

The second instrument is a combination of a harmonium reed and an organ pipe, through which the air is driven. They are arranged so as to sound the same note when pure air is used, so that when there is a lighter gas present the organ pipe sounds a higher note, thus producing beats.

So far as the experiments have gone hitherto, the first form is by far the most accurate, being capable of detecting the presence of 1 or 2 per cent. of fire-damp.

4. Note on Electrolytic Conduction. By Professor Tait.

It is commonly said that there is a resistance to a current at the surface of contact of a solid conductor and an electrolyte. Some good authorities, however, say that we have as yet no proof of this, as the effects observed may be due to polarisation. It is obvious that, if the reverse electromotive force due to polarisation contain a term directly proportional to the strength of the current, the ordinary methods of measurement would not enable us to distinguish this from the surface resistance above mentioned. For, in the expression

$$I = \frac{\Sigma(E)}{\Sigma(R)},$$

if the numerator contain a term of the form $-eI$, it may be expunged, provided e be added to the denominator.

To clear up this point I have recently made a number of experiments. These have led me to some curious results bearing on the theory of electrolysis, which I propose to bring before the Society on a future occasion. At present I refer to them merely so far as to say that they establish fully the existence of the surface resistance above mentioned. Thus I was led to see that if a slip of platinum

be inserted between the electrodes of a decomposing cell it ought, except in extreme cases, to produce almost precisely the same result as a similar and equal slip of glass or mica. This was easily verified. Here we have the singular result of a marked diminution of the current by the insertion into the electrolyte of a substance which is in itself a much superior conductor. Even when the platinum completely closes the path from one electrode to the other, so as to form two decomposing cells instead of one, a comparatively small hole made in it at once changes its function from that of common electrode to each of two decomposing cells into that of a mere *obstruction* in one cell. It is an interesting experimental inquiry to trace the intermediate stages between these two states, as a pinhole in the platinum is gradually enlarged. Whatever, then, be the behaviour of the particles of an electrolyte, they do *not* behave like little pieces of platinum.

5. Note on Thermal Conduction. By Professor Tait.

Monday, 6th May 1878.

SIR C. WYVILLE THOMSON, Vice-President, in the Chair.

The following Communications were read :—

1. On the Indications of Molecular Action in the Telephone. By R. M. Ferguson, Ph.D.

The accepted theory of the telephone represents that the vibrations of the sending plate to and from the pole of the magnet before which it is fixed is the origin of the currents generated in the pole bobbin of wire, and that these currents transmitted to the receiving telephone produce corresponding to-and-fro excursions of its plate. This theory, which is that of the inventor, may be shortly designated, in the happy words of Sir William Thomson for a kindred action, the push-and-pull theory. We have had in this session of the Society two communications of a practical nature, which seem directly confirmatory of this view. I refer to the lucid exposition of Gott's telephone experiment in the island of St Pierre, and

the beautiful and successful demonstration of the action of the phonograph, both by Professor Fleeming Jenkin. In the former of these, as we learned, one end of a thread was attached to the one side of the light suspended coil of a Thomson ink recorder, and the other to the paper disc of an ordinary mechanical telephone. This was done at the two communicating stations. When the sending disc was agitated by the voice, the coil to which it was attached twisted round in the powerful and uniform magnetic field in which it was placed, and dispatched corresponding electric current waves to the receiving instrument, the coil of which was thereby moved similarly in its field, and transferred its motion to its paper disc. A more beautiful manipulation of an exquisitely designed and executed apparatus it is not easy to conceive. In the phonograph we have as it were a mechanical telephone, with the string connecting the discs cut, and nothing left of it but the two ends stiffened into pricking pins. Instead of the sending disc dealing directly with the receiving one, its energy is employed in imprinting, by means of the pricker, its vibrations on the tinfoil, and this imprint, when again vivified by the energy of the rotating drum, reproduces the vibrations which originally stamped it.

After two such demonstrations, it may be held as proved that the electric telephone is equivalent to a mechanical telephone with an electro-magnetic intervening action instead of a mechanical one. It seems therefore a hopeless task to seek for indications of molecular action where mechanical action declares itself so manifestly. The mechanical action of the voice and of the membrane of the tympanum of the ear is above question, and that mechanical vibrations are dealt to the sending instrument, and emitted by the receiving one, is equally undoubted; but the intervening electric agency, how generated in the one and how transformed in the other, is a fair field for discussion. The action is novel, and it is surely a likely inquiry to investigate whether its explanation by the first principle that comes to hand, viz., the push-and-pull of the discs, fully covers the case. The question may be raised, for instance, whether the mere impact of the waves of air on the iron disc may not affect its magnetic condition by internal change or vibration,* so as to excite currents without vibrations of the push-and-pull kind, or whether in

* Something like this was suggested by Professor Forbes.

the receiving disc the particles may not set up an action on their own account, independently of the displacement caused by the poles of the adjoining magnet. In a mechanical telephone, we do not find that it is made to sound only by the normal push or pull of the thread, the faintest rubbing on the irregularities of its surface, either on the disc or the tube to which it is attached, makes a sound loud enough to be heard, and we can easily admit that if an internal vibratory disturbance be set up in any direction in it, the same would be audible enough. In a discussion as to mechanical and molecular sounds, it may be safely admitted, where electricity or magnetism is concerned, that any action that is clearly traceable to disturbance within a body is molecular in its origin. It will, moreover, be granted that the mere smallness of any vibration does not necessarily give any clue to its origin. Infinitesimal vibrations are not necessarily molecular, nor are vibrations of molecular source free from external motion; and we can only say that a vibration comes from molecules if we can assign to it no outside cause. It may, however, be to the point that a vibration may be assumed to be molecular because of the difficulty in suppressing it, a vibration springing from within being more independent of direction than one produced from without from one quarter.

I propose in this communication to raise such questions in regard to the telephone, and though the results obtained may not be decisive, they may be some little contribution to the discussion.

I would begin with a case where internal action seems wholly absent. I refer to the action of a tuning-fork on the telephone. It has been mentioned in more than one communication to the Society, that a tuning-fork acts best without the disc. We find that the loudest sounds are sent to the listener at the receiving telephone, when one prong is brought with its flat vibrating end in front of the core or pole pin, and next to that when the prongs, if they are not too far apart, are laid with their flat sides vertical at an equal distance on each side of the pin. When the handle of the fork is laid on the core, and held upright, the resonance of the wooden frame of the telephone and the table on which it rests becomes loud, but only a faint trace of this is sent to the distant hearer. If we magnetise two like forks, one which we may call A, to be like a bar magnet having the end of the handle as one pole, and the other

pole split in two in the two prongs; and another fork B, the two prongs of which are made like the poles of a horse-shoe magnet, with the handle an excrescence between, we find that while the fork A produces sounds alike with both prongs when held near the core, the two prongs of the fork B show a marked difference. The like pole to that of the core sounds much weaker than the other. All this is indicative of the ordinary magneto-electric induction at work.

If we detach the coil from the magnet, we have still further illustrations of the same. Both forks, A and B, sound loudest when placed with one prong on its flat side over the hollow at the centre (fig. 1), and both continue to sound, but with diminished force, as they are withdrawn in the same position from the middle to the margin of the coil. When laid with the plane of the prongs horizontal (fig. 2), they act differently. The A fork has its best sounding position when each prong lies symmetrically to the hollow axis, and it has a position of silence at a point between the middle and

Fig. 1.

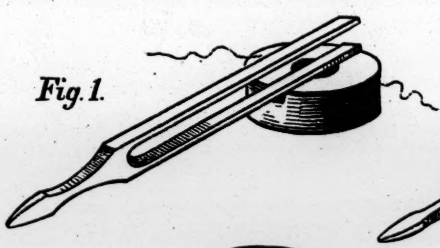


Fig. 2.

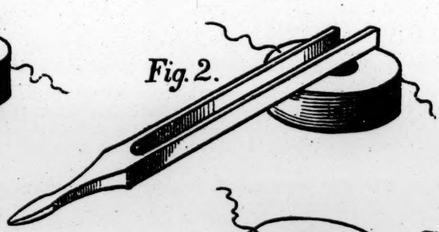


Fig. 3.

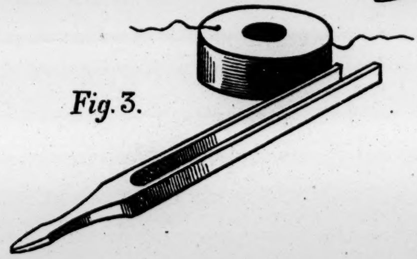
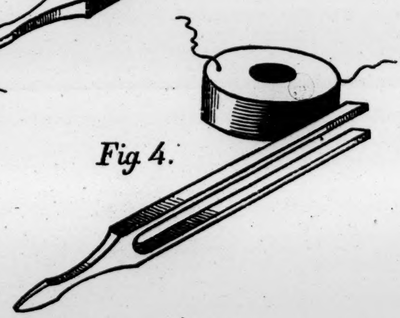


Fig. 4.



the outside, whilst the B fork in these positions acts in the opposite way. There are two positions that the forks may occupy at the side of the coils, where their similar and dissimilar actions are again shown. The first is when the plane of the fork is perpendicular to the axis (fig. 3), where both forks transmit no sound when held in the middle of the coil, but are heard when vibrating on either side. The second is when the plane is parallel to the

axis (fig. 4), where A is silent when its prongs are equidistant from the middle of the coil, and the fork B loudest. All this is, as we have said, simply an illustration of a well-known action, and at the same time a beautiful demonstration of the way in which a tuning-fork vibrates. The coils I used were of fine copper wire, $1\frac{1}{2}$ inch diameter and $\frac{3}{4}$ thick, but smaller coils would do equally well, and the forks were the ordinary small A and C forks sold by the musicsellers. It is perhaps worthy of note that a coil, a magnetised fork, and a telephone form a handy combination for testing the completeness of a circuit, as the sound of the fork coming directly to the ear is immensely below that heard in the telephone in the operator's hand. When the telephone does not sound, there is a break in the circuit. In these various performances of the fork, we have evidence enough to prove that the cause assigned by Bell for the sending action of the telephone covers at least the greater part of that action. At the same time, it must be borne in mind that the vibrations of the fork, and the sounds produced by them, are immensely greater than any connected with the telephonic effect of the voice, and that it is possible that the conditions of iron vibrating under the energy treasured up in it may be different from what they may be when the iron is beaten by the air.

But even this tuning-fork performance is not quite free from ambiguity. To find whether there might not be some change of magnetic condition due to internal vibration, able to generate currents, I cemented two coils (fig. 5.) to the vibrating ends of a large tuning-fork. It was a C (256 vibrations), with prongs upwards of 6 inches long, $\frac{7}{8}$ broad, and more in average than $\frac{1}{4}$ thick. The distance between the prongs at the end inside was $\frac{7}{8}$ inch. The coils weighed $\frac{1}{2}$ oz.; they were $\frac{3}{4}$ inch diameter, 1 inch long, and were of ·007 inch copper wire. They reduced the pitch from C to A. The cement was the hard and tough black tarry compound

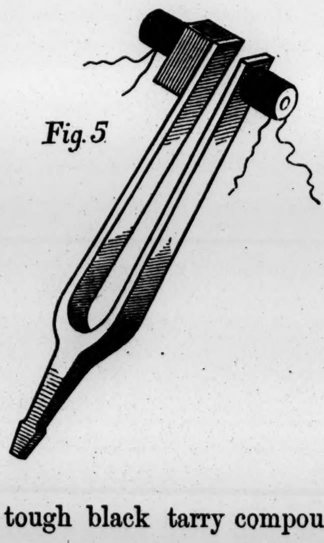


Fig. 5

used by electricians. On connecting one of the coils with a telephone and making the fork vibrate, I was astonished to hear the sound in the telephone astonishingly loud. I found, however, that this arose from the connecting wires, which though loose were able mechanically to transmit the vibrations of the fork. This was shown by leaving only one wire in the connection, when the sound of the fork was still heard. It is well known that the thread of a mechanical telephone delivers its message to the first fixed obstacle it meets and no further, and in this case, when the lines were held in the fingers, a mere residuum of sound was heard, which could only be properly estimated in a distant room away from the direct sound of the fork. In this, as in all subsequent experiments, I took the utmost care that no mechanical transmission was possible, and the telephone used at the operating station was put out of circuit each time a sound was made for investigation. Loud tapping was made in every conceivable place to ascertain if such could be transmitted in a mechanical way. The circuit was about 150 ft. of wire with a gas pipe return, and the two stations were in different buildings. The results obtained from this fork were not of any value other than illustrating the difficulty of insulating for transmission what may be looked upon as an internal and not an external vibration. They were these. The sound of the fork was heard much less loud than if the prong had been vibrating in front of the coils, but was louder than what was got when it vibrated at a distance equal to that of the prong on which the coil was not fixed. When the two coils were joined up consecutively, one way of joining gave a maximum, the other a minimum sound. In another arrangement the coil was attached to a hollow prism of thin brass $\frac{5}{8}$ inch inside, which in its turn was attached to the prong. The sound given by this coil was less than when it was attached immediately to the prong, but was louder than when the other prong vibrated freely at the same distance. When I mounted the fork in this way, one prong was feebly magnetised, and the other scarcely if at all.

A perfect maze of reasons may turn up to account for the currents dispatched by these fixed coils. One might be their motion in the magnetic field of the earth, which is not likely, for earth-induced currents proceed from the rotation of a coil, and the faint approach

to rotation in this case can hardly be looked upon as such. To exclude this cause, I put the coils in the best position for generating such currents, and connected them with a moderately sensitive reflecting galvanometer. I inserted a piece of wood between the prongs, and wedge-like drove them suddenly out, and allowed them to return, keeping time with the known pendulum-like oscillation of the mirror so as to accumulate effect, but no result was got. Again it might be that the coils, tightly as they were wound, were yielding, and we had only a case of shaking a coil in front of a magnet, instead of a magnet in front of it. That this was not the case was proved by cementing the coils to the sides of the prongs at right angles to the direction of motion, and though the forks vibrated almost unmusically with such protuberances, yet what sound they emitted was telegraphed to the listener. The most likely cause was that the coils were fixed as regarded the prongs on which they were stuck, but were affected by the distant prong which vibrated in front of them. The sounds of the vibrating coils were louder than when the coils were fixed, and the other prongs vibrated at the same distance; but that might be accounted for by the fact that the prong to which the coil was fixed was an active agent in propagating the vibrations of its companion prong to its own coil. The only chance of anything like internal action being heard here rests on the possibility that this last is not sufficient, and that is certainly a slender enough basis to build a theory upon.

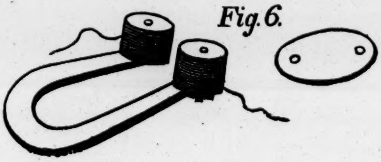
In pursuit of the same internal sending action, I cemented two coils, one to the end, the other to the middle of a bar magnet. Both transmitted telephonic currents when the magnet was struck, the end one much louder than the other. Again, an unmagnetised tuning-fork was made to vibrate above a coil, and its note was feebly yet distinctly heard at the listening end. To test whether it and the other objects afterwards mentioned were magnetised, I brought them near a small active magnet about an inch in length, and before another much heavier, 9 inches in length, and watched to see if repulsion or even indifference was shown to any part of them; and lastly, I made them vibrate in front of the coil connected with the reflecting galvanometer in the position in which their sending action was afterwards tested. These tests cannot be alleged to prove

complete absence of magnetism, but they go far beyond the practical application of the word unmagnetic. The fork in question answered perfectly all these tests. I next tried to get sounds out of soft iron plates kept fixed above the coil, and found no difficulty in getting them to sound when laid on a table in a horizontal position and beaten with a small hard wooden mallet, or with something that would produce with them a sharp tap. When the plates were held approximately in the plane of the magnetic meridian there was more difficulty in getting the distant listener to hear, but even in it they did not cease to sound under blows sharp and hard enough. They also sounded when the coils were cemented to them, but the hearer had his powers taxed to catch the effect. Soft iron pins placed in the hollow axis of the coil also sounded under similar treatment. I could not succeed, however, in getting a soft iron pin perfectly to answer the galvanometer test, ordinary nails and similar iron pins always caused a slight deflection, which became more marked by pendulum accumulation. Even a soft iron pin, as pure as I could get, after being exposed for half an hour to a white heat, on being made to move at right angles to the dipping needle, at least so nearly as this position could be given to it by the hand, indicated through its coil faint traces of magnetism. I had some difficulty in getting taps out of it for the listening station, but I succeeded at last by using another similar pin held in the same line as a mallet. Iron thus softened seems to lose its telephonic power more by absence of elasticity than by being less magnetic, for the said pin seldom failed in any position before being annealed, and so far as tests went it was not a bit more magnetic. A somewhat curious result was got by cementing a small coil to a strip of ferrotype plate quite near to the edge, and making that edge strike on the teeth of a syren wheel, made to revolve on a turning table, when the screeching note thus produced was distinctly telephoned. When there was any slight indication of magnetism, such as was given by the spot of light shifting even a millimetre for one quick motion, the taps were rendered with great certainty. The soft iron pins just mentioned, when put on the pole of a magnet, and very gently tapped with a common pencil, made themselves distinctly heard. This can be done in the case of any telephone on removing the disc.

Now, it may be asked, are all such sound-exciting currents to be traced to the shifting of magnetic matter in a magnetic field? or is there not ground for believing that a blow for the instant disturbs the magnetic condition of a magnet, and magnetises soft iron, in consequence of a disturbance of the molecules, enough to excite telephonic currents? And may not such magnetic changes act without the displacement in the field that generally or perhaps necessarily accompanies them, and heightens their effect? Wiedemann has shown that blows alone given at right angles to the magnetic needle, and consequently apart from the earth's magnetism, produce permanent magnetic changes, and that torsion and change of external conformation may do the same. The permanent effect also of blows in lessening the powers of a magnet, or of giving to iron under induction a magnetic set, has long been known. Now, may not the telephone reveal to us that they in every case produce a momentary effect of an intensity in some degree proportionate to the magnetisation of the iron?

The following telephonic arrangement, which is different from that of Bell's, may possibly have some bearing in the view just suggested; but knowing, from the instances discussed, the ambiguity that attends a departure from the push-and-pull theory, I quote it more as a problem for push-and-pull solution, than as a proof of blow-

Fig. 6.



induced effects. The apparatus (fig. 6), consists of a horse-shoe magnet rather more than 5 inches long, with a pin on each pole standing at right angles to the plane of the magnet. The pins are provided each with a coil of fine copper wire, the two being joined up as in an electro-magnet. The pole pins are $1\frac{1}{2}$ inch apart, and a stiff disc (No. 28), 3 inches diameter of untempered steel, is screwed to them by large-headed screws. Such a disc, when aided by a wooden or non-metallic resonating box, can both convey and receive an articulate message, indistinctly certainly, but still it does it. When it is joined up with the galvanometer, and tapped with a pencil on any part, taking care of course not to combine the

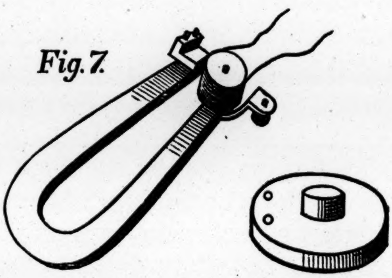
striking and lifting motions, no visible deflection is got, though the same taps on the disc of a Bell telephone make deflections of from 5 to 10 mm. of the scale. When the disc is taken off, we may ascertain the efficacy of vibrations in the line of the plate to generate currents. Taking a small thin iron rod and giving it small vertical excursions, I found that when these were made in the line of the plate no effect whatever was got when the rod was held equatorially to the line of poles, but that as the rod was brought to either pole an increasingly slight effect was seen. This might be due in part or perhaps wholly to the want of parallelism of the motion to the pins, for the slightest side or horizontal motion produced a marked effect. That even this stiff plate vibrates when spoken to there can be little doubt, but the vibrations cannot, on the ordinary push-and-pull principle, push and pull at right angles to themselves. The action of the disc may be due to the blow it gives to the pole pins as an ordinary resonator, and possibly also the action of blows just hinted at may be present. The aerial blows may directly induce currents, or they may do so in altering the shape of the plate by vibration. However this may be, the problem may safely crave a solution from the push-and-pull theorists. When the plate is not screwed down, but laid or gently pressed on the pins, and more especially if it be made the bottom of a shallow tin box with a hole on the top, its performance almost rivals that of Bell's telephone. The improved effect is no doubt due to facility of vibration which is as necessary to the sounding power of vibrations of molecular origin as to those produced mechanically.

I made an attempt also to see whether the coil itself had any sounding action. If a blow can make soft iron induce a current, possibly a closed coil, the electric analogue of soft iron, may do so under the same treatment. I first tried this by hitting the side of a coil by a small wooden hammer, then as the frame of it gave way under this usage, I cemented it to a piece of board and hit the board; but as again the whole coil split up under the blows, I lastly tied it to a strip of wood, and used it as the head of a hammer against the end of a wooden rod. In each case I succeeded once or twice in telephoning the taps, but most unaccountably, for I might tap ever so hard and so often afterwards without being again so fortunate. How the sounds were sent in these cases I cannot say. The coils

used, which were the same as those employed with the tuning-fork, could send with a brisk turn of the hand a current induced by the earth, may have been so excited, but there was next to nothing rotational in the blow they got or gave.

It may be well believed that many of the sounds thus produced in the telephone were very faint, and required a sharp ear and undivided attention. I have mentioned only these results which have been repeatedly got. Possibly some of them may be wrong as regards comparative loudness, but I had in almost every case the impression of different listeners. The listener was provided with two telephones of the following kind. One pole of a horse-shoe magnet (fig. 7), carried the bobbin pin and a brass arm for an adjusting screw. To the other an iron stage was fixed, to which was screwed a box $3\frac{1}{2}$ inches in diameter and $\frac{1}{4}$ inch deep, with a hole in the top $\frac{3}{4}$ inch wide, provided with a short tube for insertion into the ear. The bottom of this box was of untempered steel plate (No. 28), and its distance from the pin could be adjusted by the screw. Such an instrument told faint tuning-fork sounds, which the ordinary one pole ferrotype plate telephone I had passed over.

Fig. 7.



Leaving now the sending station, we proceed in our search for molecular action to the receiving instrument, and we examine the sounds emitted by its electric and magnetic parts. Instead of having a person performing the irksome task of talking or singing at the sending end, let us lay aside the distant telephone, and put in its place and that of the speaker a Bunsen cell charged with water and a mercury break. Such a cell would have no effect on the telephone without the break, for it is only momentary currents that affect the instrument. The break thus furnishes us with an intermittent or discontinuous current, and is in fact an untiring and uniform speaker, uttering a series of ticks or taps which are very audible to the distant telephone hearer. Now let us begin with the coil which is the "fountain and source" of all action in the receiver. Let us detach it from the core, and hold it up to the

ear. By attentive listening a faint ticking is heard, and if we wish to hear it without strain we must add another cell or two to the battery. The sounds emitted by the coil are very faint, and cannot be excited by the voice or even a tuning-fork. With small battery power the coil may be made to sound, but with a strong battery it may be heard at some distance from the ear. Thus the coil which we thrashed almost to death to send a message receives one without demur. We now insert in the coil a piece of soft iron, and we have a duet of two undistinguishable ticks, the principal part being performed by the iron. Here again the receiving action is immensely better than the sending, and leads us to suspect that our rough efforts to send were mere bungling, due to our ignorance of the right way to do it. If now we bring a magnet and place the soft iron pin on its pole, or screw it into the pole, a sudden bound is made in the loudness of the sound, and with the one cell water current we can hear a few inches off. Whatever vibrations were effected in the pin by itself now grow immensely more pronounced when it is made magnetic. We may now replace the coil in its core in the telephone, for our arrangement is nothing else than a telephone without a disc. The sending and receiving powers are now more nearly on a par, for we found that the gentle tap of a pencil can now be sent. Let us now, in imitation of Professor Tait and Mr Blyth's experiments, put a piece of glass or pasteboard, or wood or other non-conducting substance, on the core. The sound waxes thereby louder, as was explained by the plate acting as an ordinary resonator. Lift any one of these the smallest thing from the core, and it acts as a dead wall to stop the sound of the core. In place of these, let us take a series of conducting metal plates of lead, German silver, copper, and silver, all equally thick and as nearly as possible of the same temper. When we place one of these on the core we hear the same resonance as in the non-conductor, but we also perceive what appears to be a new and separate action set up by the plate on its own account, and as we change from disc to disc we find that the sound grows with the conductivity of the plate till we reach silver, which is very audible. When now we put an iron disc of similar size and temper, a sound is heard which completely eclipses the loudest of the others. All these discs when held slightly away from the core sound louder than when touching it, and the cul-

minating receiving action, where sending and receiving are on a par, is the disc of iron in front of the pole, almost but not quite touching it—in fact, the disc of Bell's telephone.

Let us now retrace our steps, and go over the ground more minutely, beginning again with the coil. The coils I used consisted of from 30 to 100 ohms of copper wire ·007 inches, and were of various sizes and shapes. For such fine wire coils, as already stated, one cell will suffice if the listener be sharp and attentive, but two or three must be used for easy hearing. The intermittent current given off by an ordinary medical electro-magnetic machine produces very audible sounds in such coils. The electric wave of such a machine is peculiarly adapted to excite telephonic sounds. It is a double wave of opposite name, with a sharp beginning and an equally sharp termination. For thick wire coils a more powerful electromotor is required. With five fully charged Bunsen cells, I managed to get every coil I could lay hands on to render the ticking of the break, whether they had iron cores or not. The sounds were perfect telephonic performances; for it mattered not whether the wire was thick or thin, covered with silk or wool or cotton, the tick was not at all musical, but simply the reproduction of the sound of the break. We should be inclined at once to call such a rendering molecular, did we not know that in this era of mechanical telephones and phonographs, discs of tinfoil, oiled silk, paper, potato, butter, and other unlikely substances, can reproduce the tones of the human voice without peculiar accent. Coil ticking or tapping is familiar to any one who has dealt with a powerful induction coil. The sole of the instrument resounds with the primary coil rendering of the break outside the instrument.

Now, whence comes this sound in a coil? Wiedemann, who gives De la Rive the credit of first noting the sound, attributes it to the clashing of the various convolutions against each other, due to the known action that wires conveying currents in the same direction attract each other. With a view to answer this question, I wound up 15 feet of ·04 copper wire into a spiral $\frac{3}{4}$ of an inch in diameter, and sent the intermittent current of a five-celled fully charged Bunsen battery through it. On holding the end of the spiral to the ear, I heard the tapping distinctly. On drawing out the spiral, so that no two

convolutions were in contact, I failed to hear any sound; but when I put two or three convolutions together and held them to the ear, the sound was again heard. I now isolated one convolution, and tied the thread of a paper telephone to it, and, although there was no neighbouring convolution in contact, I heard, by means of the telephone, the ticking distinctly, and, when the spiral was drawn out into a plain wire, the same sound continued. The current, as indicated by a tangent galvanometer of a single hoop of stout copper rod, produced a deflection of 54° for the unbroken current, and of 41° for the intermittent one. The cell resistance was under two ohms, and the exterior resistance under one ohm. The nitric acid was old, and had been frequently used. Removing the ·04 wire from the circuit, and substituting a short length of ·024 copper wire, the current interruptions were heard still better in the paper telephone. I suspected mechanical transmission of the sound, but that could scarcely be, the wires being led from the open air by a fixed course under the floor. When all mechanical connection was broken through mercury troughs, there was no alteration; and when a loop of 2 inches of ·024 wire was held firmly in a vice it did not cease to sound. I need not, however, have been so sceptical, for sounds of a similar origin had been got as early as 1845 by Beatson, De la Rive, and other observers. They may not, without the aid of the mechanical telephone, have got in the wires the same clearly rendered series of ticks that we hear by its aid. De la Rive's description of them, however, is both definite and graphic.*

* His words are:—"Quant au son lui-même, je ne peux pas mieux en donner une idée, qu'en le comparant à celui qu'on produit avec la roue de Savart. C'est une suite de bruits résultants du choc des particules métalliques les unes contre les autres, beaucoup plus qu'un son musical. On entend aussi, il est vrai, des sons musicaux. Ce sont les harmoniques du son que rendrait la tige ou le fil par l'effet des vibrations transversales; ils proviennent du mouvement vibratoire qu'éprouve le métal, mais ne sont pas un effet direct de l'influence électrique à laquelle il est soumis. On peut en effet, les faire disparaître en touchant avec la main le corps vibrant, sansque pour cela disparaisse le bruit fondamental."—"Le son que rend un fil de fer bien recuit quand il transmet le courant est un son très fort, qui ressemble beaucoup au son des cloches d'église dans le lointain. On pourrait peut-être l'employer avec avantage dans les télégraphes électriques."—"Le ton du son varie avec la vitesse avec laquelle les courants discontinus se succèdent; quand cette succession est très rapide, le son ressemble beaucoup au bruit que fait le vent lorsqu'il souffle fortement."—*Comp. Rend.*, 1845.

He details his experiments, and, unlike other observers, extended his researches to other metals besides iron. He stretched wires of various metals on a sonometer with a helix of wire round them, so that he could excite sounds by the simple passage of the current in the wire, or by the magnetising action on it of the helix. He used an electromotor of five Grove's cells. He tells us that the sounds emitted from iron and other metals, by the direct passage through them of a discontinuous current, were in no way different from those obtained by magnetisation when the same current was made to pass through the helix surrounding the wire. This was especially observable in the case of iron. He states also loosely that the loudness of the sounds emitted with the same strength of current was in proportion to the resistances offered by the wires, and that possibly this sounding action had the same conditions as heat in the galvanic circuit, so far at least as resistance was concerned. He, moreover, states that iron stood quite exceptionally among the metals in its power to give out such sounds.

The exact way in which De la Rive obtained these sounds appears to have been lost, for no subsequent observer accords with him. Wiedemann, for instance, in his "Electro-Magnetismus," in keeping with the investigations of Wertheim, states that iron alone emits sounds under the above conditions, and he quotes De la Rive's remarks on other metals within parentheses. Now, the simple device of attaching the string of a mechanical telephone to the wires when the intermittent current circulates, enables us to observe these sounding effects with perfect ease and certainty. When attached to a copper wire (·007 inches diameter) the sound is very marked; and when to an iron wire (·008) the telephone sounds many feet off, the current being that from the five-cell Bunsen battery.* Indeed, for the iron wire a telephone is not necessary, for the ticking can be heard when the wire is held in the teeth, or when it is doubled on the finger and inserted in the passage of the ear. There is only one objection to the last mode of procedure, viz., that the wire is almost too hot to be comfortable. It is not, however, necessary to insert the wire itself into the ear, for a cotton thread, tied to the wire and placed in the ear, sounds nearly

* This was distinctly heard in the auditorium of the Society when the paper was read.

as loud. A thin iron wire is therefore the most rudimentary form of a telephone. Gott's telephone has thus another explanation in addition to that of the mere twisting round in a magnetic field; for if only the coil be left without a field, or even if only one convolution of it be left, and held in the chaps of a vice, its power of moving the paper disc would not wholly disappear.

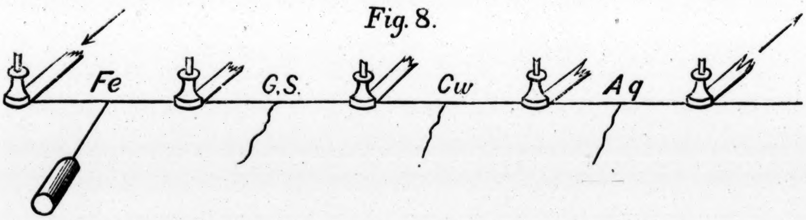
When the paper telephone was attached to the thin iron wire, and the distant break worked by the hand of an operator, a loud sound was heard when the dipper of the break entered the mercury, and another equally loud, but not louder, when it was lifted out. In this case the break was two rooms off from the listener, at a distance of from 30 to 40 feet, and when the operator was made to say "in," "out," at each motion, short as the distance was, the wire sounded before the voice. The sounds thus given had a clear metallic ring about them. This, however, disappeared when the wire was coiled tightly round the finger. The mechanical telephone enables us to hear such sounds in all portions of the wire without any interruption. We can hear, for instance, the half musical sound in a straight wire, the dull unmusical sound when the wire is tightly coiled, and the increase of this when several convolutions of a covered wire are kept tightly together. In the single wire, although the sound is loudest when it is tense, yet it is distinctly heard when perfectly loose. De la Rive tells us that the resistance in the sounding wire must be greater than that of the remaining circuit, battery included. I have found that this is not necessary, for I can hear distinctly the sound of a thin iron wire when the current is the weak one furnished by a single Bunsen cell charged with water. The Bunsen cells mentioned here have an active medium surface of 40 square inches. Again, with a small magneto-electric machine for medical purposes, almost any experiment can be demonstrated.

With reference to the strengths of current necessary to excite the wires, the characteristic of the telephone mentioned by Professor Tait, in his communication on the "Strength of a Telephonic Current," must be borne in mind, viz., that the telephonic effect does not depend so much on the actual strength of the sounding current as on the rapidity with which changes in the strength are effected. The break used in these experiments made from five to six interruptions in the second, that certainly was not very

sudden. Then, in a mercury break, the spark that ensues on the separation of the dipper from the surface of the mercury is conducting, and it may only be the break of the attenuated current when the spark ceases that has telephonic power. This would bring us down to a low scale of effective action, when working with a water cell interrupted six times per second. It cannot, however, be with certainty alleged that this kind of interruption is strictly single, as there may be pulses in it preceding the final stoppage.

The relation of resistance to sound can be shown by an experiment like the following:—I measured off a yard of iron, German

Fig. 8.



silver, copper, and silver wire, of diameters respectively of ·008, ·0086, ·007, and ·006 of an inch. The resistance of each yard was respectively 3, 8·7, ·7, and 1·4 ohms (the silver alloy was exceptionally hard). From these again I cut off a foot and soldered them end to end in a successive chain, and stretched them out (fig. 8) in a line, with their junctions firmly clumped to prevent the sound of the one running into the other. I then attached four similar mechanical telephones to them, and found, at least so far as the ear could judge, that the sounds emitted by each, when the same broken current passed through all, were proportionate to the resistance such offered, in the case of the last three metals, whilst the sound given out by the iron distanced all the others. The German silver here was much louder than the silver, which again was, one could fancy, twice as loud as the copper. This seems to clash with what we have stated with reference to the conducting discs on or near the core of the magnet of the telephone. It may be somewhat hazardous to venture an explanation, but there is one that turns up so readily at the first consideration of the matter that I may mention it. In the case of the wires the same current passed through all, but in the discs the currents induced would be, as is generally received, relatively in pro-

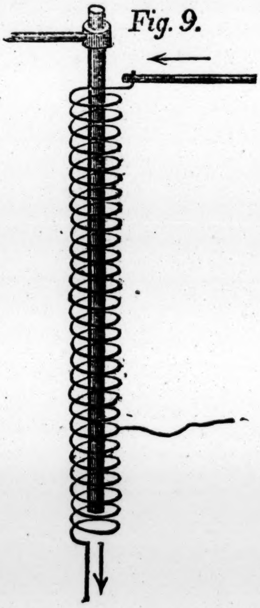
portion to the conductivity of each. If it now be accepted that the sounding action be like heat proportionate to the square of the current strength, the apparent discrepancy is accounted for. Suppose the conductivity of silver to be ten times greater than that of German silver, then in the silver plate a tenfold greater current would be induced, and would have in consequence one hundredfold greater sounding power than that of the German silver in the same resistance; but as the resistance of the German silver is ten times greater than that of the silver, there would still be a tenfold louder sound in the silver than in the German silver, quite a sufficient margin to remove the discrepancy. In addition to the action within the plates here discussed, there may of course also be the external push-and-pull, as with the iron disc. In wires the length involved has little effect on the sound; for though a long stretch of wire does sound louder than a short one, the difference is by no means in keeping with the respective lengths. In thick wires no sound is got. I tried in vain with the strongest current I could conveniently use to get a sound out of a No. 14 copper wire.

Now here at least it will be admitted that we have a molecular action. It may be replied that the earth's magnetism has something to do with it. To show that it has not, we may try the somewhat inelegant feat of holding the end of the thin iron wire in the teeth, and turning it in every possible direction, when we shall find that the loudness of the ticking has no relation to direction. Again, when the string of the telephone is tied to the wire and made to go round it in a circle, we shall find no maximum or minimum points. This then is a telephonic action absolutely free from external push and pull, proceeding undoubtedly from molecular disturbance in the wire. That the action is magnetic in some way is evident from the exceptional position of iron in the series of metals. De la Rive held that the sounding action of an iron wire, when the seat of a discontinuous current was almost identical with, and must be traceable to the same ultimate cause as, that it displays when placed in a magnetising helix excited by a similar current. The parallelism between the wire telephone and the ordinary electric telephone is so striking that no theory of the latter can be held to be complete without it includes the former. It seems to suggest that the sounds we hear in plates and rods under sudden electric or

magnetic changes is partially at least due to the molecular disturbances set up by induced momentary currents. Be that as it may, surely no one would be inclined to say that the ticking of a Bell telephone under a series of momentary currents is wholly mechanical, and that of a wire telephone under the same conditions wholly molecular.

After observing these sounds, my next inquiry was to ascertain how they comported themselves in a helix capable of motion. With this view I coiled about ten feet of fine iron wire (·0173 inch diameter) into a spiral about $\frac{3}{4}$ inch in diameter. I suspended it (fig. 9) by a thick wire held on a stand, and arranged that its lower end should dip into a vessel of mercury. Such a spiral is exceedingly delicate, and the up and down motion of its slender convolutions is most easily excited. When left free to itself, and there is no interruption in the mercury below, that is, no spark reaction, the motion produced by a discontinuous current of five Bunsen cells is very slight; but if a $\frac{3}{8}$-inch soft iron rod be introduced so as to be clear of the sides of the spiral, the mechanical excitement of the spiral is more marked, but still not much. But if I hold down the lower end, and it so happen that the tension and number of convolutions be sufficient, the helix divides itself into two very active ventral segments with a node in the middle. The motion, especially at the middle of each segment, is now very considerable. But if it does not vibrate thus symmetrically, by shifting the fingers a little, a point is easily found by which at least one very active ventral segment is secured. If now to a point of maximum motion I solder a very fine copper wire, to serve as the thread of a mechanical telephone and make it come out at right angles to the motion of the spiral, it does not interfere with it. By this contrivance I secure the advantage of listening to what goes on in the wire without interfering with any mechanical magnetic effect. Profiting by the experience of the fork, I found that a fine wire could hang loosely, and yet convey vibrations to

Fig. 9.



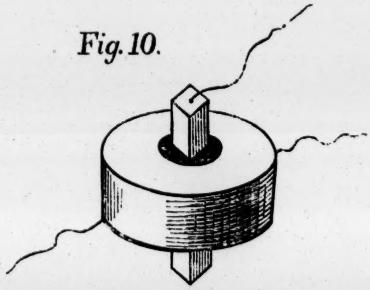
the telephone. Fine iron wire answers very well, but in this case it is inapplicable, as it sets up a clearly audible action on its own account. When the ear is now applied to the telephone the ticking that you would hear in the wire, if it were straight, is heard distinctly, and is the only distinct sound, for the up and down motion of the convolution only produces a slight jingling in the connecting wire, and possibly also in the coil. When the convolution is held tight in the fingers the ticking goes on if anything more distinctly than before. Whether the mechanical motion by the increased fixity of the helix is converted into louder ticking, could not be decided by the difference. Here we have two vibrations quite distinct from one another, the ticking in the wire and the mechanical motion due to the mutual electro-magnetic action between the rod and the coil. If this last were quick enough it would also be telephoned, but the rate of vibration being below that of musical frequency it is nothing but inaudible motion. In this helix action I would submit we have a dissected view of all receiving telephonic action—a vibration *in* the body clearly of molecular origin, and another *of* the body of a push-and-pull kind. The latter may be stopped or nearly so, but not the former. From its internal origin it is bound to make itself good, and when the body is held in the grasp of the most rigid substance it only propagates in it the minute vibrations which no elastic matter can stop. In the vibrations of coils, cores, or plates, the same thing holds. Molecular vibration is present in them all, and how far mechanical vibration chimes in in unison depends entirely on the ease with which such can be produced.

There is apparently a marked difference between these two vibrations. The one can make itself good acoustically by one impulse, the other not. I have tried in vain, while holding taut the thread of a mechanical telephone without letting the fingers slip, to produce, by a sudden pull or let go, the tick that resounds in iron or a conductor when a current suddenly begin or ends, and I could only do so by letting the string slip the slightest degree, so as to set up a short series of vibrations. Each galvanic or magnetic tick or tap may not, in the first instance, be more than a simple shock; but so sudden is it that the particles concerned do not recover at once, but continue vibrating for an infinitesimal time, and hence

the pitch or musical sound that generally accompanies it. When, however, a prolonged vibration is difficult, and the impulses brace each other up by frequent repetition, we have possibly in the first case a very short series, and in the other only one vibration. Such vibrations are as capable of rendering all complex acoustic combinations as vibrations of the push-and-pull kind.

It would be a matter of mere speculation to guess how the conditions of the vibrating helix are transferred to vibrating rods or discs of iron in an intermittent magnetic field. I would only say that the same double vibration is clearly traceable in them. To illustrate this in the case of rods, I took a small coil of No. 20 wire, 2 inches in diameter and about an inch high with a hollow axis of $\frac{3}{8}$ inch, and sent the interrupted current of a five Bunsen cell battery through it. Inside the axis I put a soft iron pin 2 inches long and $\frac{1}{4}$ inch square (fig. 10). To the upper end a fine copper wire was soldered to act as the thread of a telephone. When rightly placed the pin supported itself in the hollow, and kept dancing up and down symmetrically without much friction against the inside of the bobbin hollow. Here the mechanical motion was not so clearly eliminated as in the case of the helix; yet the ticking was heard, and it alone, when the motion of the pin was stopped by the hand. The impossibility of stopping the ticking of the pin was

Fig. 10.



shown by securing its ends between the jaws of a vice and making it as tight as possible, when the vice itself took up the tale of electric interruption, and made itself heard all round. A curious change was observed in long iron rods when this coil was placed round the middle of them and when shifted to the end. In the former position the sound was a staccato rendering of the longitudinal note of the rod, and in the latter this sound was lost in a dull tapping. In

the latter position there was a pronounced tendency to push and pull.

I adopted a similar arrangement with the vibrating disc of the telephone. To the middle of one I cemented an india-rubber tube to act as a yielding handle, and the paper telephone wire was soldered near the middle. The disc was made to move in front of a telephone core excited by the coil just named. The movements of the disc were very violent, and made in all directions, making the connecting string jingle loudly, so that the isolation of the ticking sound was not so satisfactory as in the two previous cases. Still it was heard, and when the motions of the disc were kept in one direction its loudness did not grow with the extent of the motion. With a fine coil and a water cell the ticks were distinct enough; but the mechanical displacement was too small to yield the required comparison. The impossibility of stopping this ticking was illustrated in the following way:—A ferrotype plate was held tightly between two thin pieces of plate glass, the space between being filled up with sealing wax so that the whole was a solid mass of glass and wax. This was brought near a core excited by a water cell, when the sound was loudly rendered. Another illustration to the same effect was that of cementing by sealing wax the ferrotype plate of a telephone to a disc of thick microscopic glass, and putting this in the telephone with the glass side to the ear or mouth. Its articulate functions, though much impaired, still continued.

Lastly, to test whether the tick in a coil was due to electric conduction, I screwed a pin into the core of the telephone so as to act as a prolongation of the core; round this I placed a coil of fine wire, to which the string of a paper telephone was attached. There was no ticking heard so long as the circuit of the coil was broken; but the moment it was closed the ticking began. The coil was, of course, clear of the core. At the same time, however, there was mechanical action between the coil and core, illustrating the difficulty in such cases of determining by direct observation whether the single mechanical pull may not also make itself heard.

In conclusion, I would say, by way of summing up the evidence of this paper, that at the sending station the evidence of molecular action, though suggestive, is by no means conclusive; while at the receiving station the existence of molecular as well as mechanical

action amounts to demonstration. How the actual performance of the receiving instrument is to be apportioned between these, it is, of course, difficult to say. Taking into account Professor Tait's calculations as to the infinitesimal strength of a current that can make a telephone tick, and assuming that that tick is purely molecular, as we have done, molecular action must be there not the less considerable.

2. Sketch of the Arrangement of Tables of Ballistic Curves in a medium resisting as the Square of the Velocity, and of the Application of these Tables to Gunnery. By EDWARD SANG.

The motion of a body in a medium whose resistance is proportional to the square of the velocity, has been the subject of many inquiries. Its intimate connection with the theory and practice of gunnery has produced for it the attention of almost every cultivator of the higher analysis; but, like several other seemingly elementary problems in mechanics, it has hitherto received no complete solution.

If nothing else than the fluid's resistance influence the motion the investigation is comparatively easy; thus, taking the time as the primary variable, the speed has its own square for its derivative, and so must be proportional to the first inverse power of the time, and consequently the distance passed over must be proportional to the logarithm of that time. Hence, if the present position, the velocity and the coefficient of resistance be given, we can compute, forwards, the position and velocity at any future time; or backwards, what those had been at previous times. But since the velocities are inversely proportional to the times elapsed from some fixed epoch, it follows that,

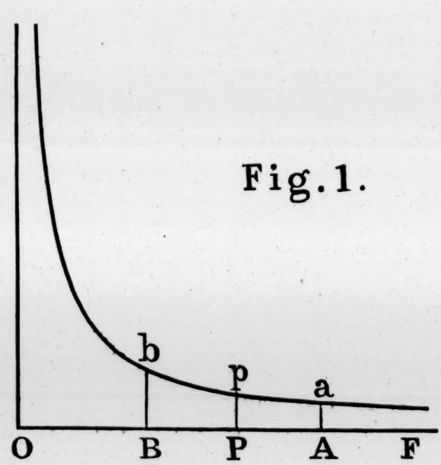


Fig. 1.

at that epoch, the velocity must have been infinite, so that although the body may have come from an infinite distance, setting out therefrom with an infinite velocity, it must have begun that motion a finite time ago.

If we represent time by distances measured along OF, one of the asymptotes of a hyperbola, the ordinates, such as Pp, drawn parallel to the other asymptote, are proportional to the corresponding velocities. Thus the present velocity being Pp, that at the future time OA will be Aa, while that at the former date OB had been Bb; and the areas BbpP, PpaA represent the distances passed over during the intervals of time BP and PA. The distance corresponding to the finite previous time OP is thus infinite, and so also must have been the velocity of projection at the date O.

When the motion is affected by some influence other than the resistance, the investigation becomes more intricate. The case of a constant gravitation in a fixed direction is the simplest of these complications, and the simplest case of this is when the directions of the motion and of gravitation coincide.

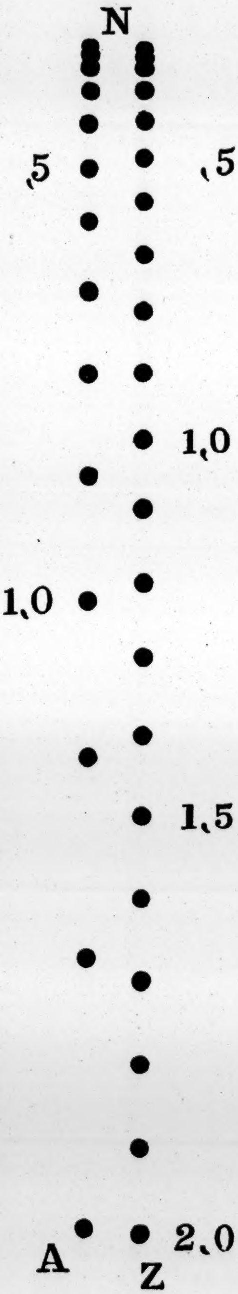
If a stone be thrown straight upwards, its motion is impeded both by its weight and by the air's resistance; in the subsequent descent the motion is accelerated by gravity, but retarded by the air; so that, for the ascent, the soliciting influence takes the form $g+cv^2$, and for the descent becomes $g-cv^2$. Now the change in the sign of the velocity from $+v$ in the ascent to $-v$ in the descent, is not accompanied by any change in the sign of v^2, and therefore both parts of the motion cannot be represented by any one algebraic formula. Accordingly we find the upward motion to be represented by circular functions; the downward motion by the corresponding catenarian ones.

In fig. 2, the left hand row of dots represents the upward motion graduated to equal intervals of time. The stone is first shown at A as having come from some indefinite distance below; its speed, rapidly diminished, is altogether extinguished at N. In order to avoid confusion, the descent is shown on the adjoining right hand column of dots.

In descending, the acceleration due to gravity becomes less and less; it would cease altogether if the velocity could become so great as to cause a resistance equal to the weight; the tendency, there-

fore, is to reach a definite terminal velocity, and the stone ultimately moves almost uniformly. If it had been thrown downwards at a rate greater than this terminal velocity, its motion would have been retarded, but less and less so as it approximated to the same limit of uniform motion.

Fig. 2.



We may study the ascent by tracing it backwards from the highest point, fancying the air to have then the quality of hastening the motion. In this case the velocity would increase to become infinite; but this infinite velocity would be acquired in a finite time. In fact, the time being represented by a circular arc, the velocity would be proportional to the tangent of that arc, so that in the time corresponding to a whole quadrant, the velocity would become infinite. Thus it seems that, however rapidly a stone may be thrown upwards, its motion is extinguished within a finite time determined by the coefficient of resistance.

Each particular body has its own terminal velocity depending on the weight and on the extent and peculiarities of the surface exposed to resistance; but the motions of all follow exactly the same law, so that one diagram may serve for all, the units of comparison alone needing to be changed.

Also, one table of the positions and velocities may be made to do for all cases. In the arrangement of such a table we have to seek for the most convenient system of units; now, on contemplating the motion of a projectile independently of our measures of time and distance, we perceive that the terminal speed is the only standard with which we can compare the velocities at the various parts of the path, wherefore we adopt this terminal velocity as the tabular unit of speed.

This terminal velocity has to be considered along with the intensity of gravitation, which intensity, if acting alone, would generate velocities proportional to the times; wherefore the time in which the heavy body, falling freely, would acquire the terminal velocity, presents itself as the proper tabular unit of time, and, as a necessary accompaniment, the tabular unit of distance must be that over which the body would pass with its uniform terminal velocity in the time during which that terminal velocity was acquired; or, in other words, must be double the height of the free fall needed for the acquisition of the final velocity.

The accompanying table has been constructed for these assumed units; the details of the rise are given on the left hand, those of the fall on the right hand side, the times being reckoned before and after the instant of culmination:—

Time.	Velocity.	Rise.	Fall.	Velocity.	Time.	Time of Free Fall. / Time of Actual Fall.
−0·0	·00000	·00000	·00000	·00000	+0·0	1·00000
·1	·10033	·00501	·00499	·09967	·1	·99918
·2	·20271	·02013	·01987	·19738	·2	·99669
·3	·30934	·04569	·04434	·29131	·3	·99265
·4	·42279	·08223	·07795	·37995	·4	·98714
−0·5	·54630	·13058	·12011	·46212	+0·5	·98026
·6	·68414	·19197	·17014	·53705	·6	·97221
·7	·84209	·26809	·22727	·60437	·7	·96313
·8	1·02964	·36139	·29075	·66404	·8	·95321
·9	1·26016	·47544	·35983	·71630	·9	·94260
−1·0	1·55741	·61573	·43378	·76159	+1·0	·93143
·1	1·96476	·79055	·51194	·80050	·1	·91988
·2	2·57215	1·01512	·59369	·83365	·2	·90806
·3	3·60210	1·31864	·67850	·86172	·3	·89608
·4	5·79789	1·77215	·76570	·88552	·4	·88393
−1·5	14·10143	2·64878	·85544	·90515	+1·5	·87204
			·94681	·92167	·6	86005
			1·03968	·93541	·7	·84824
			1·13381	·94681	·8	·83637
			1·22898	·95624	·9	·82516
			1·32500	·96403	+2·0	·81394

Opposite the time 1·3 in the first column, we find the velocity 3·60210, this means that if the stone be thrown upwards with the

velocity 3·60210, as at A in fig. 2, it will reach its highest point in the time 1·3, the height to which it will rise being 1·31864, as shown in the third column. The subsequent fall is shown in the fourth column, and there we find that the same level is reached just before the time 2·0; the whole time of flight being somewhat less than 3·3.

To translate this into ordinary measures, let us suppose a body whose terminal velocity is 320 feet per second; the time in which this velocity is acquired in free fall is 10 seconds, and therefore all the tabular times must be multiplied by 10, the tabular velocities by 320, and the linear distances by 3200. Hence if such a body were thrown upwards with a velocity of 1152 feet per second, it would rise in 13 seconds to a height of 4230 feet, and would thence descend in 19·93 seconds, striking the ground with the velocity of 308 feet per second.

This table enables us to interpret easily the results of experiments made on falling bodies. Thus, if the height and the time of descent be accurately observed, we may thence deduce the terminal velocity; from which, again, knowing the weight and the extent of surface, we may discover the constant of the coefficient of resistance. In order to facilitate the computations for this class of experiments, we may annex another column to our table, namely, one containing the ratio of the time of free fall to the actual time of fall. Thus the fall through the distance 1·32500, which is done in the time 2·0 with resistance, would be accomplished in 1·62788 if there were no resistance due to velocity, and the ratio, as shown in the annexed column, is ·81394.

Suppose now that a body has been dropped from a height of 400 feet, and that the observed time of the fall is 6 seconds; the time of falling freely from this height is known to be 5 seconds, and therefore the ratio is ·83333. The table shows that this ratio belongs to the tabular time 1·8271, and that, consequently, 3·284 seconds is the time in which the terminal velocity is acquired in falling freely; that terminal velocity, therefore, must be 105 feet per second.

In experiments of this kind, the disengagement at the beginning and the stroke at the end of the fall may be made, by help of the

electro-magnet, to record themselves alongside of the records of the beats of a clock, and thus very great precision may be obtained.

When the stone is thrown obliquely, its path is curved; were there no air, that path would be the well-known parabola, which forms the first ideal approximation. With speeds very small in comparison with the terminal velocity, the deviation from the parabolic curve is slight; but the deviation becomes excessive in the ordinary practice of gunnery.

Beginning with the case of a body thrown obliquely upwards, we observe that its upward motion is resisted both by the air and by gravity; when it has reached its highest point and is descending, the downward motion is less rapidly accelerated than the upward motion had been retarded, so that the culminating point is reached earlier than the half-time of flight. On the other hand, the horizontal transference is retarded all along, wherefore the vertex of the curve is beyond the middle of the horizontal range, as is seen in fig. 3, which shows the positions of a projectile at equal intervals of time.

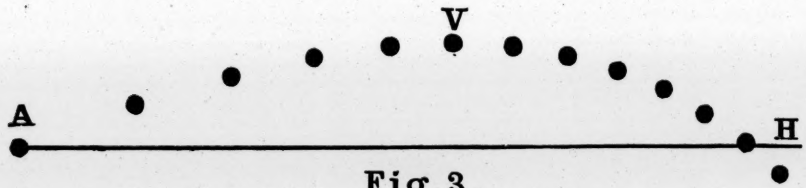


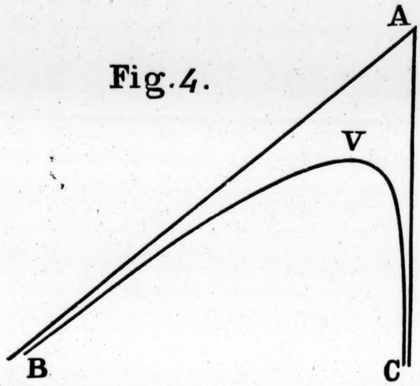
Fig. 3.

After having passed the highest point at V, the projectile bends its motion more and more downwards, until ultimately the path almost coincides with the plumb-line; the speed, at the same time, gradually approximates to the terminal velocity, and thus the curve in that direction is limited by a vertical asymptote.

Reckoning backwards on the other side of the culminating point, the speed is rapidly augmented, and would become infinite within a definite time; the inclination of the curve also tends to a definite limit, wherefore this branch, too, has a rectilineal asymptote; so that the whole curve is continued in the angle formed by the two asymptotes as represented in fig. 4. Thus the branch VB, unlimited in

length, is limited in the time of its description; while the branch VC, also unlimited in length, is unlimited as to time, but is limited as to the extent of its horizontal range.

Fig. 4.



The intensity of gravitation being known; if the terminal velocity, the direction, and the velocity at any point be given, we can compute the position, the velocity, and the direction at any proposed instant. These three data serve to determine the curve, and in general any three data are sufficient, as, say, the inclination, the range, and the time of flight. The direct computation from the first-named three arguments is very operose; in the other cases it is much more so, because we can only proceed by the method of successive trials, or, what comes to the same thing, we must have recourse to tabulated results.

My object at present is to describe the arrangement of tables of this kind, and to explain their uses.

If we launch two masses at the same angle with speeds proportional to the terminal velocities, the paths are similar in shape though on different scales and performed in different times. This circumstance is the first and principal guide in the arrangement of the tables, because the computations for the one trajectory are easily made to serve for the other. We naturally select for tabulation that one in which the terminal velocity is unit.

These ballistic curves differ in character as well as in size. The characteristic of the shape may be taken as the angle A (fig. 4), made by the two asymptotes, or we may adopt the velocity at some definite direction, say the velocity at the summit V. The former is general in its application, it includes the cases of projection obliquely

downwards, even to the limit of projection in the direction of gravity; but the latter has the advantage of more ready reference to practice, and lies more to hand in the calculations.

In the subjoined example of a table, the velocity at the summit is assumed to be just equal to the terminal velocity, and the details are given for intervals of one-tenth part of the tabular unit of time. For actual use these intervals must be made much smaller, and the tables should be arranged for values of the characteristic velocity, differing slightly from each other; the former table may be regarded as the beginning of the series with the title V = 0.

Since the curves extend indefinitely both ways, such tables must always be incomplete; we can do no more than carry them to the limits of probable utility.

The first part of the table contains the details of the rise; thus a body shot off at the instant − ·50, with the velocity 2·18, and at an

Velocity at Summit, V = 1·00000.				
Time.	Elevation.	Velocity.	Rise.	Hor. Distance.
·00	0°··00′	1·00000	·00000	·00000
− ·10	+ 5 ··25	1·11630	·00518	− ·10537
− ·20	+10 ··12	1·27188	·02158	− ·22323
− ·30	+14 ··17	1·48161	·05092	− ·35716
− ·40	+17 ··42	1·77176	·09580	− ·51262
− ·50	+20 ··27	2·18190	·16041	− ·69840
Time.	Depression.	Velocity.	Fall.	Hor. Distance.
·00	0°··00′	1·00000	·00000	·00000
+ ·10	− 6 ··00	·91394	·00484	+ ·09521
+ ·20	− 12 ··25	·85219	·01883	+ ·18217
+ ·30	− 19 ··03	·81041	·04127	+ ·26201
+ ·40	− 25 ··42	·78508	·07157	+ ·33561
+ ·50	− 37 ··08	·77348	·10921	+ ·40362
+ ·60	− 38 ··38	·77521	·15481	+ ·46631
+ ·70	− 44 ··48	·79801	·20628	+ ·52326

elevation of 20°··27′, gradually loses speed, as shown in the third column, and reaches the summit at the instant ·00, having there a velocity of 1·00, and being now ·160 above its original level. The details of the descent are given in the second part of the table. On

consulting the column marked Fall, we find that the original level is reached between the instants +·60 and +·70, by interpolation at ·611, so that the whole time of its flight has been 1·111. Also, in the columns marked Hor. Distance, we find ·6984 for that covered during the rise, and ·4726 for that passed over during the fall, making the total horizontal range 1·1710. The velocity with which the ball strikes the ground is seen to be ·7777, while the impact is at an angle of 39°··19′. The squares of the initial and final velocities are nearly in the ratio of 55 to 7; that is to say, of the work done by the gunpowder in putting the ball in motion, 48 parts are spent on the air, and 7 parts only remain to represent the destructive effort.

Thus we can readily compute the range, the time of flight, and the incidence of the ball. A table of these, such as the following, forms a convenient adjunct to the fundamental table :—

Velocity at Summit = 1·00000.					
Elev.	Velocity.	Range.	Time.	Velocity.	Depr.
5°··26′	1·11630	·20372	·20347	·91142	6°··13′
10 ··12	1·27188	·41740	·41452	·84497	13 ··22
14 ··17	1·48161	·64538	·63470	·79993	21 ··26
17 ··42	1·77176	·89405	·86649	·77594	30 ··01
20 ··27	2·18190	1·17100	1·11087	·77768	39 ··19

In order to apply these results to business, we must ascertain the values of the tabular units in terms of the actual units of time and distance. This is easily done if the terminal velocity be known. As an example, let us take a bullet whose terminal velocity is 800 feet per second, in which case the tabular velocities must all be multiplied by 800. A heavy body falling freely acquires velocity at the rate of 32 feet per second for each second of time, and would acquire this velocity of 800 in 25 seconds, wherefore all the tabular times must be multiplied by 25. Lastly, the unit of distance is described with the unit velocity in the unit of time, wherefore $800 \times 25 = 20000$ feet is the actual linear unit of the tables as applied to this particular projectile. The above example, therefore, expressed in English feet and in seconds of time, becomes

Angle of elevation,	=	20°··27′.
Initial velocity,	=	1745·5 feet per sec.
Time of ascent,	=	12·5 sec.
Rise,	=	3208 feet.
Time of descent,	=	15·6 sec.
Horizontal range,	=	23420 feet.
Final velocity,	=	622 feet per sec.
Angle of incidence,	=	39°··19′.

If the same ball be shot from the same gun, but at another inclination, the shape of the path is changed and the details thereof must be sought for in another table. We search among the various tables for that one in which the given initial velocity is found opposite the proposed angle of elevation; if the tables be constructed for values of V sufficiently close, we shall find this either directly or by an easy interpolation; and then, proceeding as above, we can get all the desired information.

Similarly, if the initial velocity and the horizontal range be given, we convert these into the corresponding tabular numbers, and search among the various tables for that one in which these two are found together; the angle of inclination, the time of flight, and all the other quæsita of the problem are then to hand.

It has been stated that three data suffice to determine the path. When the terminal velocity is one of these, the solution is obtained by simple inspection; but when that velocity is one of the quæsita, the operation becomes indirect.

Suppose that the velocity communicated to a given shot by a specific charge of gunpowder has been ascertained, say, by help of the ballistic pendulum, we may discover the terminal velocity of that shot by observing the angle of elevation and the horizontal range. For this purpose we assume some terminal velocity, thence compute the corresponding tabular initial velocity, and thereby obtain the corresponding horizontal range. If this come out too much, we must reduce the assumed terminal velocity, and continue our trials until the computed agree with the measured distance. If the time of flight have also been carefully noted, we get a corroboration of the accuracy of the result.

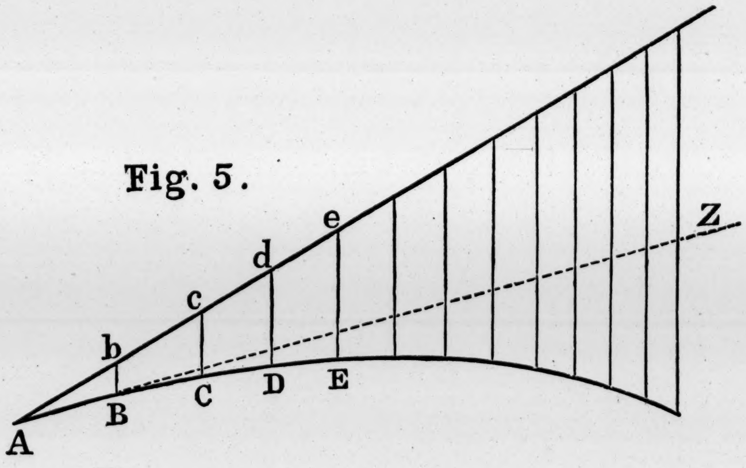
Not only so, we may dispense altogether with the ballistic pen-

dulum, and determine both the initial velocity and the resistance by accurate observations of the angle, the range, and the time of flight. For the purpose of facilitating the solutions of the various problems which may arise in practice, auxiliary tables may be derived from the fundamental ones.

When a shot is fired in a steady breeze, the direction in which the ball meets the air is not the apparent direction of the gun, it is that of the resultant of the two motions, and the computations have to be made as for that resultant.

Thus if AZ (fig. 5) represent the horizontal direction of the gun, and AB the initial velocity of the ball projected on the horizontal plane, while *b*B represents the velocity of the wind, A*b* will be the horizontal direction in which the ball meets the air. Since the vertical motion is not affected, the tangent of the elevation of the gun must be changed in the ratio of A*b* to AB in order to get the tangent of the true angle of elevation in relation to the air, and A*b* multiplied by the secant of that elevation is the true initial velocity.

Fig. 5.



If we now, using these corrected arguments, compute the horizontal distances corresponding to equal intervals of time, measure those along the prolongation of A*b* and from the successive points draw parallels *c*C, *d*D, &c., multiples of *b*B, we shall have the

horizontal projection of the ball's path, that path being a line of double curvature.

As an example of the variety and completeness of the information conveyed by such tables, we may cite the path detailed in the preceding table and represented by fig. 3.

The ball is projected from A with the velocity of 2·18, and at once encounters a resistance $4\frac{3}{4}$ times greater than its weight. The speed is rapidly lessened, and the path is deflected to become horizontal at V, where, in the present instance, the resistance is just equal to the weight. On account of this resistance the speed is still slackened, but gravity now comes to accelerate the motion downwards, and, at about the fifth interval from V, has overcome the retardation, thereafter the velocity slowly increases, and tends ultimately to reach the limit 1·00.

Those cases in which the characteristic angle A of fig. 4 is obtuse have little or no application to gunnery; in them the path is never horizontal, but is inclined downwards all along.

The analysis of these motions is complex, and the calculations thereon following are tedious, but the results, when tabulated, are of easy application. The theory would be uninteresting to those engaged in the actual business, just as the mode of construction of trigonometric and logarithmic tables is scarcely ever thought of by the navigator or surveyor. What we have at present to consider is the advantage to be gained by the compilation of a series of tables such as those sketched out.

3. On some Physical Experiments relating to the Function of the Kidney. By David Newman, Glasgow. Communicated by Professor M'Kendrick.

(*Abstract.*)

This paper treats of the physical influences which promote the secretion of urine, as far as can be demonstrated by experiments upon animal membranes and the kidneys of animals recently killed. Before going on to consider the subject I may be permitted simply to mention the theory held regarding the means by which the

kidney performs its function, and also say a word or two in connection with the structure of that organ. As regards its histology the kidney may be said to be composed of two elements—(1) the blood-vessels, and (2) the tubuli uriniferi. (This is leaving out of account the lymphatic arrangement.) The kidney receives its supply of blood from the renal artery, which, as it passes into the substance of the kidney, penetrates the cortical portion and gives off branches. The uriniferous tubules in this part of the kidney end in globular dilatations called the capsules, or Malpighian bodies; it is into these that the branches of the renal artery pass to form convoluted coils, the glomeruli. The branches of the renal artery which pass into the glomeruli are called the afferent vessels, and the vessels that are formed by the reunion of the branches of the glomeruli are called the efferent vessels. After the efferent vessels emerge from the capsule of the Malpighian body they again subdivide to form true capillaries, most of which go to form a closely meshed network round the tubuli uriniferi. They finally unite to form the radicals of the renal vein.

To make use of the description of Mr Bowman, "it would be difficult to conceive a disposition of parts more calculated to favour the escape of water from the blood than that of the Malpighian body. A large artery breaks up in a very direct manner into a number of minute branches, each of which suddenly opens up into an assemblage of vessels of far greater aggregate capacity than itself, and from which there is one narrow exit. Hence must arise a very abrupt retardation to the velocity of the current of blood." But besides this arrangement, by which a large volume of blood is exposed to circumstances the most conducive to free filtration of its fluid constituents, we have a condition, namely, the secondary capillary system on the distal side of the glomerulus, which, by its resistance to the onward flow of the blood, subjects the blood inside the Malpighian body to considerable pressure. It is now generally supposed that the excretion of urine takes place by filtration of a dilute solution of the soluble constituents of the urine through the glomerulus into the capsule of the Malpighian body. This weak solution then passes along the tubuli uriniferi, where it comes into close contact with the blood it has just left. It is then supposed that an interchange takes place between the blood in the capillaries surrounding

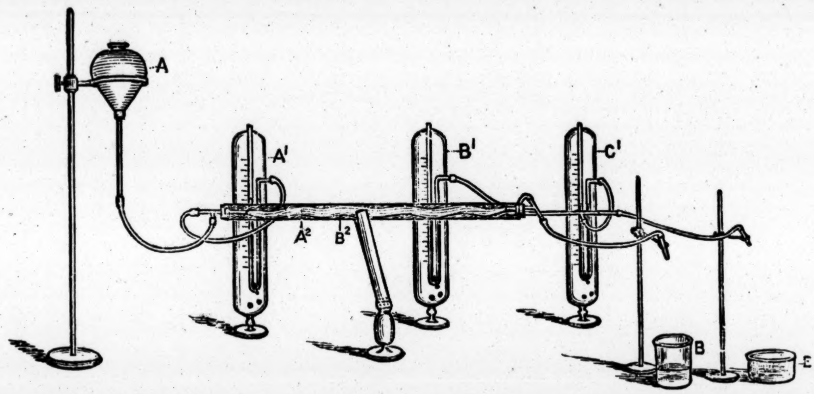
the tubules and the fluid inside by which a certain amount of the water passes again into the blood, and so leaves the urine in a more concentrated state than it was when it first passed from the glomerulus into the dilated end of the urine tubes (Ludwig). It is believed, however, that the epithelium, which lines the convoluted tubes, performs certain functions in connection with the secretion of the solid constituents.

The rapidity of the secretion of urine may be said to depend upon the following factors:—(1) The relationship which exists between the pressure of the blood in the glomerulus of vessels and the urine in the capsule of the Malpighian body and in the tubuli uriniferi; (2) the state of the blood pressure in the venous system of the kidney; (3) the pressure upon the lymphatics; (4) the quality of the blood in the artery of the Malpighian tuft; (5) the state of the walls of the artery constituting the Malpighian tuft, and of the capsule itself, these being regarded as the filter through which the fluids and soluble constituents of the blood have to pass. The influence of vaso-motor nerves upon secretion must not, however, be forgotten; not only do they exert an influence upon the quantity and quality of the secretion by dilating or contracting the arterioles, but their influence upon the chemical processes, by reason of their communications with the secreting cells (Pflüger), must be remembered; (6) activity of tubular epithelium.

In the experiments I have endeavoured to imitate the conditions found in the Malpighian body of the kidney. I have not been able, however, to represent the lymphatic arrangement.

The apparatus, a drawing of which, kindly executed for me by my friend Dr Robert Moffatt, is shown in the following woodcut. It consists of a piece of rabbit's bowel A^2 enclosed in a glass tube B^2. To each end of the bowel a small glass T-tube is attached. One of those tubes is connected with a pressure-bottle A, and a manometer A^1. The pressure exercised upon the fluid inside the bowel by the pressure-bottle and indicated by the manometer A^1 will be designated the *afferent pressure.* The tube attached to the other side of the bowel conveys the fluid that passes *along* the bowel to the vessel B, and the pressure exercised upon the fluid which it contains, called the *efferent resistance,* is indicated by the manometer B^1. Through a cork at the right hand side of the large tube, another

T-tube communicates, on the one hand, with the inside of the large glass tube containing the bowel, and, on the other, the vessel E and manometer C^1. The vessel E will therefore contain the fluid that filters through the membrane A^2.



The index in the manometer A^1 will therefore represent the *afferent pressure*, and correspond to the arterial tension of the renal artery; B^1 will indicate the *efferent resistance* (when applied to the kidney, the venous resistance), while the manometer C^1 will show the resistance offered by the fluid in the tube B^2 to the transudation of the fluid inside the bowel, and therefore correspond to the tension of the urine in the capsule of the Malpighian body.

In the first series of experiments water was passed into the bowel under an afferent pressure of from 10 to 50 mm. of mercury, the efferent resistance being the same in each experiment as the afferent pressure, so that no water passed along the bowel to B. The amount of fluid which transuded through the bowel was found to increase in accordance with the pressure used. It was shown that for every 10 mm. increase in the pressure, there was ·533 c.c. more water filtered through the bowel per minute. Thus under a pressure of 10 mm. 2·133 c.c. transuded in a minute, and when a pressure of 50 mm. was applied 4·266 c.c. passed in the same time. From these results we would therefore conclude that the amount of fluid which transudes through an animal membrane is increased according to the tension of the fluid inside. In relation to this experiment, take the following:—Instead of an animal membrane the kidney of a recently killed horse was employed; in this case a three-quarters per cent. salt solution was used instead of water, as it

was found that when water is passed into the vessels it almost immediately passes through the walls and causes œdema of the tissue, and the onward flow of the fluid is prevented. The salt solution seems, however, not to pass through the walls of the vessels into the lymphatic spaces so readily if the kidney is quite fresh, but still it passes from the glomeruli into the dilated end of the tubuli uriniferi. I found it very difficult to get this experiment to work satisfactorily, as the kidney requires to be used immediately after the death of the animal, and a number of precautions need to be taken which it is not necessary to mention here.

In the experiments alluded to the salt solution was passed into the artery under various pressures, the venous resistance being equal to 20 mm. in all except the first, in which case no resistance was offered to the exit of the fluid by the vein. In the first experiment the solution seemed simply to pass from the arterial into the venous system, very little being pressed into the urine tubules. When, however, the efferent resistance is raised to 20 mm., and at the same time the afferent pressure advanced to 40 mm., the increase in the amount of fluid pressed into the ureter is obvious. In the other experiments upon the kidneys of animals, the results of which I will not give in detail, a somewhat similar plan was adopted. The following are the results:—(1.) When the fluid contained in the ureter is subjected to pressure, the quantity of fluid that passes from the vein is diminished in relation to the pressure employed, and so also is the amount of fluid that transudes from the glomerulus into the capsule of the Malpighian body. (2.) The quantity of fluid that passes from the vein depends upon the amount of afferent pressure; the greatest increase takes place between 40 and 50 mm. (3.) The temperature of the fluid affects the rapidity of the flow through the vessels and the quantity that transudes into the tubuli uriniferi. The higher the temperature the greater is the amount of fluid passed from the ureter, and the more rapid the circulation through the vessels. (4.) When the fluid is pressed into the artery, it finds its way readily into the vein, but when injected into the vein, it does not escape by the artery. There must, therefore, be some arrangement in the kidney, probably in the Malpighian body, by which regurgitation of the fluid is prevented.

The results of the experiments with the bowel show (1.) that

the amount of fluid that transudes is in accordance with the pressure upon the fluid inside. (2.) For every 10 mm. increase in the afferent pressure ·275 c.c. more fluid transudes per minute, and the flow along the bowel is increased; whereas, when the efferent resistance is increased, the amount of fluid that transudes is augmented by ·31 c.c. in the same time, and the flow along the bowel is diminished. Therefore the afferent pressure may be said to be expended in two ways—increasing the amount of fluid that transudes, and the quantity that passes along the bowel—but the efferent resistance exerts its whole force in pressing the fluid through the membrane, therefore, 10 mm. increase in the efferent resistance has more effect than the same increase in the afferent pressure, and for the same reason we would suppose that a given increase in the venous resistance would conduce more to rapid secretion of urine than the same increase in the arterial pressure, unless when the venous resistance is extreme when other factors come into play. (3.) The addition of urea slightly retards the transudation of water through the membrane. The filtrate contains the same percentage, whatever pressure may be employed, as the original solution. (4.) Albumen also retards the transudation of water, but it differs from urea in this respect, that the percentage of albumen in the filtrate is in relation to the pressure. The higher the pressure the larger the quantity of albumen in a given amount of the filtrate. (5.) The presence of urea in a solution of albumen assists the filtration of the albumen at the expense of the urea. The following table shows the results:—

Pressure in mm.	Albumen.		Urea.		Quantity of Solution.
	With Urea.	Without Urea.	With Albumen.	Without Albumen.	
10	·173 grms.	·131 grms.	6·5 C.grms	7·1 C.grms	14·2 c.c.
20	·460 „	·332 „	10·8 „	13·5 „	27· „
30	·750 „	·600 „	16·8 „	19·5 „	39·5 „
40	1·306 „	·900 „	22·3 „	26· „	52· „

(6.) The higher the temperature of the solution the more rapid the transudation of the fluid. Thus, when water was passed into the bowel at a temperature of 15·9° C., and under a pressure of 45 mm., 142·5 c.c. filtered through in thirty minutes; whilst, when

the temperature alone was raised to 34·2° C., 197 c.c. transuded in the same time.

At the beginning of this paper I referred to Ludwig's theory regarding the secretion of urine, namely, that the blood is subjected to a high pressure inside the glomeruli, a free filtration into the dilated end of the tubuli uriniferi takes place, and this filtrate, which is at first very dilute, gradually parts with a portion of the water that holds it in solution. This is believed to take place by a process of diffusion between the fluid in the tubuli uriniferi and the blood in the veins surrounding them on all sides. Now, if it were not that albumen retards to a certain extent the passage of crystalloids (salts and urea) through an animal membrane, then the fluid in the inside of the urine tubules would be of the same concentration (in crystalloids) as the blood. But it has been shown that when a solution of albumen and urea are filtered through an animal membrane under pressure, the filtrate is less concentrated than the original solution, particularly as regards the amount of albumen, but also to a slight extent the urea. This is more especially the case when the pressure is not great. If the blood contained nothing but crystalloids (urea and salts), then the fluid inside the tubuli uriniferi would be the same as the fluid circulating in the vessels, and no diffusion would take place during the passage of the urine from the glomerulus to the pelvis of the kidney. This is, however, not the case; the blood circulating in the vessels contains a large quantity of albumen, and, if the theory above stated be correct, more urea than the fluid in the tubules, so that, putting aside any special function the epithelium may have, diffusion must result, and a portion of the water in the tubules pass back again into the blood. This diffusion will take place as the urine passes along the tubuli uriniferi either till it becomes of the same concentration as the blood outside, or makes its escape into the common ducts that convey it to the pelvis of the kidney. Therefore, the longer the urine remains in the tubuli uriniferi, other things being equal, the more concentrated will it be.

4. Note of a Method of Studying the Binocular Vision of Colour. By John G. M'Kendrick, M.D.

There are several well-known methods of mixing colours, such as the superposition of two spectra or of different parts of the same

spectrum—the method of reflection, Czermak's modification of Scheiner's experiment, the use of rotating disks having coloured sectors, and the direct mixture of coloured powders or coloured liquids. In all of these cases the effect may be seen with one eye, and is due to the action of light on a definite portion of one retina. But may sensations of mixed colours be produced by binocular vision of the components? Regarding this question various well-known observers have arrived at completely opposite results. Thus, as mentioned by Helmholtz in his "Optique Physiologique," p. 976, H. Meyer, Volkmann, Meissner, Funke, and he himself fail in obtaining the sensation of the resulting colour, whilst Dove, Regnault, Brücke, Ludwig, Panum, and Hering state the reverse. In his great work, Helmholtz describes various methods by which he investigated the question, and his opinion amounts to this, that we have no true binocular perception of colour. According to him we may have a resultant sensation of a particular kind, different from that of the two components, but also unlike the sensation of the mixed colour obtained by methods appealing to one eye only.

In studying this subject I lately devised the following simple arrangement:—Take two No. 3 eye-pieces of Hartnack's microscope, or any similar eye-pieces of considerable focal length, and place one before each eye. If they be somewhat diverged, two luminous fields will be seen, and by adjustment, the edge of the one luminous field may be caused to touch the edge of the other. In these circumstances a definite area of each retina is illuminated. By converging the eye-pieces, the two fields may then be partially overlapped, and when the axes of the two eye-pieces are parallel, both fields coincide. It will then be found that the overlapped portion is intensely luminous, whilst the other portions become less luminous, as if cast into shadow. By increasing or diminishing the amount of convergence of the eye-pieces, the extent of the luminous field may be varied at pleasure, and the two fields coincide when the two images fall on the two yellow spots. If, then, a small piece of coloured glass be inserted into each eye-piece, say red into one and blue into the other, on repeating the experiment as above mentioned, I find that the overlapping portion of the two fields gives a sensation of the resultant colour. I have repeated the experiment with various coloured media, such as coloured gelatine paper, coloured

paper rendered translucent by oil, &c. In showing the experiment to others, I have found that certain people do not see the resultant colour, whilst others do so readily. The cause of this and of the opposite statements of the observers above alluded to, I believe to be this: The sensation resulting from the fusion in the brain of the two impressions, one coming from each eye, appears to be capable of decomposition by a mental effort. Thus, the purple produced by red and blue appears as such to my eye so long as I simply look at it without any conscious effort; but if I wish to analyse it, I then find that the two colours, red and blue, seem to be superposed on each other, and the one appears to shine through the other. On ceasing to make any effort, they again fuse together as before. Again, by thinking of the colour opposite the right eye, say red, the field ceases to be purple and has a decided tinge of red, and on thinking of the colour before the left eye, say blue, the prevailing tone of the field is blue. Apparently, then, if corresponding points of two retinæ be simultaneously stimulated by two different colours, the impressions are fused in consciousness into the resultant colour; but the resulting sensation may be decomposed by an act of attention. The decomposition is effected partly by strongly directing the attention to one eye, and less strongly to the other, and the result is a sensation corresponding to the colour placed before the eye to which the attention is most strongly directed. Some of the same facts may be studied with the aid of the stereoscope.

The following Gentlemen were duly elected Fellows of the Society:—

Alex. Macfarlane, M.A., B.Sc., 2 Roseneath Terrace.
Samuel Drew, M.D., D.Sc., Chapelton, near Sheffield.
George M'Gowan, 24 Argyll Place.
James Brunlees, Vice-Pres., Inst. C.E., 5 Victoria Street, Westminster.
John Grahame Dalziel, 95 South Street, St Andrews.

Monday, 20th May 1878.

DAVID STEVENSON, Esq., C.E., Vice-President, in the Chair.

The following Communications were read:—

1. On the Genus Rhizodus. By Dr R. H. Traquair.

(*Abstract.*)

In this paper the author first sketches the history of Rhizodus, from its discovery by Ure of Rutherglen to the most recent papers on the subject.

Placed by Agassiz among the "Coelacanthi" (*i.e.*, cycliferous Crossopterygia of modern nomenclature) it was classified by Professor Huxley in the cycliferous division of his family Glyptodipterini, along with *Holoptychius*, *Glyptolepis*, *Dendrodus*. The discovery by the author, in 1875, of its subacutely lobate pectoral fin revealed the fact that it was much more closely allied to *Rhizodopsis* than to *Holoptychius*, and that it ought, along with the former genus, to be classed in a family (Cyclodipterini) distinct from the acutely lobate Holoptychiidae. The author does not consider the identity of Leidy's genus *Apepodus* with *Rhizodus* as proved.

M'Coy admitted two species of Rhizodus—*R. Hibberti*, Agassiz, and *R. gracilis*, M'Coy, merging with the former of these the *Holoptychius Portlockii* of Agassiz. An examination of the types of *Holoptychius Portlockii*, preserved in the Museum of Practical Geology, shows, however, that this species is not only specifically but also generically distinct from *Rhizodus Hibberti*, as the teeth are devoid of the cutting edges characteristic of *Rhizodus*, and in this particular, as well as in the minute striation of the surface, they closely resemble those of Hancock and Atthey's genus *Archichthys*, to which the author proposes to transfer it, at least provisionally.

An investigation of the large store of *Rhizodus* remains from the Gilmerton ironstone, belonging to the Edinburgh Museum of Science and Art, shows the presence of two well-marked species of Rhizodus, differing in the bulk to which they attained respectively, as well as in the relative thickness of the scales, the shape of many of the

bones, and the external sculpture as well of the bones as of the scales. For the larger of these two species, there can be no doubt of the propriety of retaining the specific name *Hibberti*, as it is clear that its enormous laniary teeth, as occurring in the limestone of Burdiehouse, and at first considered by Hibbert to be reptilian, originally suggested the name *Megalichthys Hibberti* to Agassiz, which he afterwards altered to *Holoptychius Hibberti*, after eliminating the Saurodipterine remains previously confounded with them, and to which latter he then limited the term *Megalichthys*, rather unfortunately, as "*Megalichthys*" is the smaller fish! M'Coy's *R. gracilis* is certainly a synonym of *R. Hibberti*, the apparent greater slenderness of the dentary bone being due to the infra-dentary plates (not known to M'Coy) being, as is often the case, awanting in the specimen; and as regards the greater slenderness of the anterior laniary, a large series of teeth from Gilmerton shows every possible amount of gradation in that respect. For the smaller species, whose remains have hitherto been confounded with *R. Hibberti*, the author proposes the name of *R. ornatus*.

1. *Rhizodus Hibberti*, Ag. sp.

This species must have attained a gigantic size, a detached dentary bone in the Edinburgh Museum measuring no less than 25 inches in length. Externally the cranial bones are ornamented by a rather fine tuberculation, the tubercles more or less confluent with tortuous ridges. The mandible displays the same structure as in *Rhizodopsis*, the *dentary* element being narrow, pointed behind, thick in front, where it carries the anterior or symphyseal laniary tooth, the three other laniaries behind being borne upon separate *internal dentary* pieces. Below the dentary, and forming the lower margin of the jaw, are three *infra-dentary* plates, while posteriorly the articular region is covered by a large plate representing the angular element. Other determinable bones described in this paper are the *maxilla*, which, as in *Rhizodopsis*, only bears small teeth, the principal *jugular*, the *operculum*, the *clavicle*, the *infra-clavicular;* there are others also whose place in the skeleton is not easily determinable. The *clavicle* and *infra-clavicular* are not tuberculated like the cranial bones, but ornamented with delicate reticulating ridges and pits; the posterior superior angle of the infra-clavicular

is produced upwards and backwards in a long slender process. The scales are comparatively thin and very large, sometimes, as noticed by Hibbert, attaining a diameter of 5 inches; usually they occur in a broken condition. Their attached surface is marked by a central boss and concentric lines of growth. The outer surface, very rarely seen, is ornamented by closely set granules, which towards the posterior border of the exposed area are confluent into wavy ridges terminating in the margin. These seem certainly to be the scales attributed to *R. Hibberti* by M'Coy, but not by Young; they are probably also the same as the undescribed '*Phyllolepsis tenuissimus*' of Agassiz.

R. Hibberti occurs throughout the Cementstone and Carboniferous Limestone series of Scotland, the most noted locality being Gilmerton, near Edinburgh.

2. *R. ornatus*, sp. nov. Traquair.

To this species, which seems never to have attained anything like the dimensions of *R. Hibberti*, belong the specimen showing the pectoral fin described by the author in 1875, the head described by Mr L. C. Miall, and in fact nearly all the specimens in which any portion of a fish with bones or scales *in situ* is shown. The ornament of the cranial bones is somewhat similar in character to that in *R. Hibberti*, but very much coarser; the same is the case with the bones of the shoulder. Of the bones the following have been recognised by the author—*dentary*, *operculum*, *principal jugular*, *clavicle*, *infra-clavicular;* there are also others whose determination is still somewhat doubtful. The clavicle differs somewhat in form from that of *R. Hibberti*, its lower portion being narrower from before backwards. The expanded portion of the infra-clavicular is also shorter than in the larger species, but the same slender process is sent backwards and upwards from its posterior superior angle. The pectoral fin is subacutely lobate. The scales are thicker than those of *R. Hibberti*. The exposed area of the external surface is marked with short, interrupted, wavy, reticulating ridges, whose direction is mainly parallel with the posterior border of the scale; in the interval between these, more delicate ridges are seen radiating from the centre. It is apparently on a scale of this species that Dr Young has founded his description of those of *R. Hibberti*.

R. ornatus occurs in the Calciferous Sandstone series of Scotland, as at Burdiehouse in Mid-Lothian, and Pittenweem in Fifeshire; but it is especially abundant in the blackband ironstone of the Lower Carboniferous Limestone group at Gilmerton, above which horizon it has not yet been discovered.

2. On the Anatomy of a recent species of Polyodon, the *Polyodon gladius* (Martens), taken from the river Yang-tsze-Kiang, 450 miles above Woosung. Part III., being its *Viscera of Organic Life.* By P. D. Handyside, M.D.

The author proceeded with his anatomical description of the respiratory, circulatory, and pneumatic systems in this remarkable fish; referring to the differences that exist in the male, the female, the young, and the adult specimens. He also shortly noticed the alimentary and other viscera of Organic life.

Dr Handyside illustrated his paper by 24 additional drawings —including 7 microscopic views—of structure in this fish.

The fourth and last part of Dr Handyside's paper will consist of a description of the articular system and the endo-skeleton of the *Polyodon gladius.*

3. A Mechanical Illustration of the Vibrations of a Triad of Columnar Vortices. By Sir William Thomson.

4. Fourth Report of Boulder Committee, with Remarks. By D. Milne Home, Esq.

Since the last Report was drawn out and laid before the Society, the Convener has had an opportunity of inspecting a considerable number of boulders not mentioned in previous Reports. Some of these are interesting, on account not only of size, but also of shape, marks on them, and position. The Committee consider that advantage will result from a special description of these, and from woodcuts of a few.

The cases have been arranged, as in previous Reports, according to counties, to indicate the geographical position of the boulders, and enable persons desirous of inspecting them, to know where to find them.

Fig. 1 Boulder at Glenelg. View of West Side

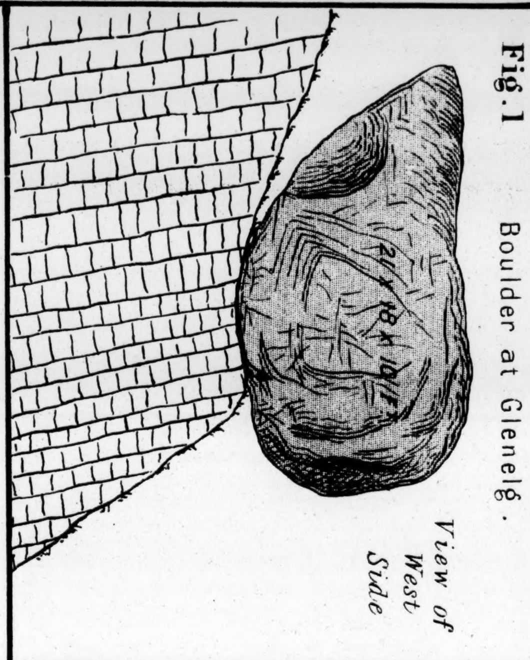


Fig. 2 Boulder at Glenelg. View of East Side

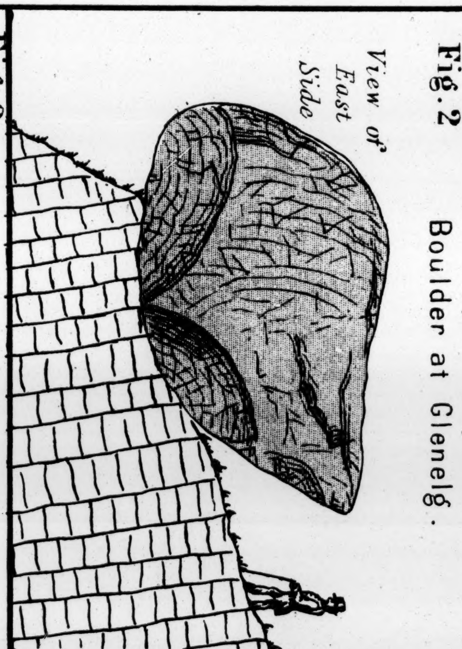


Fig. 4 Glen Rossdale.

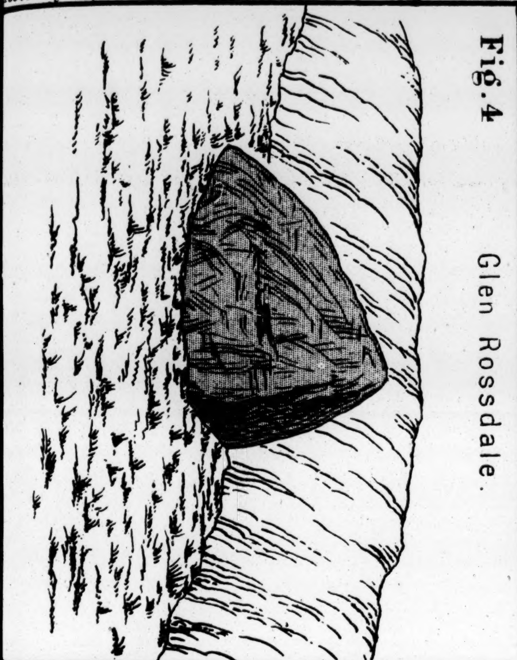


Fig. 8 Boulder at Fas-na-Cloich. Loch Creran.

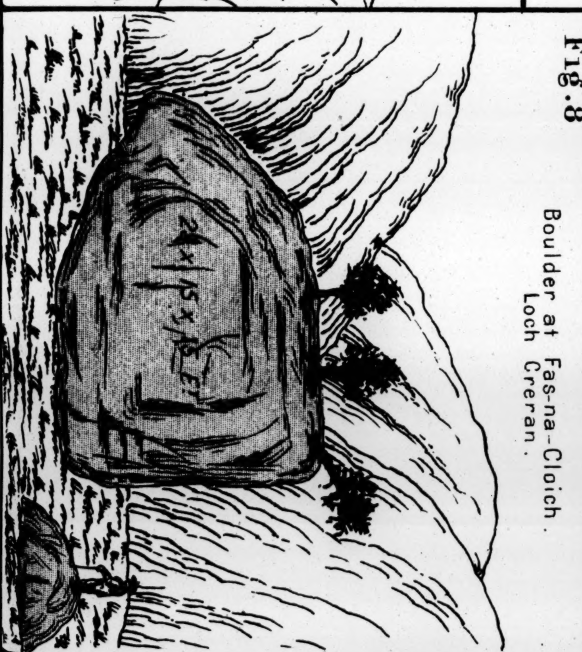


Fig. 5 Glen Rossdale. Boulders on Hill top.

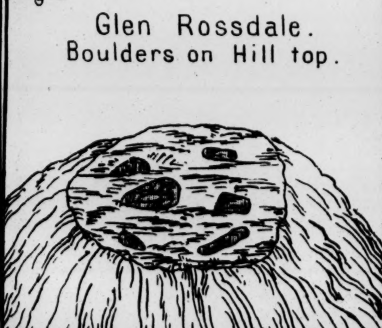
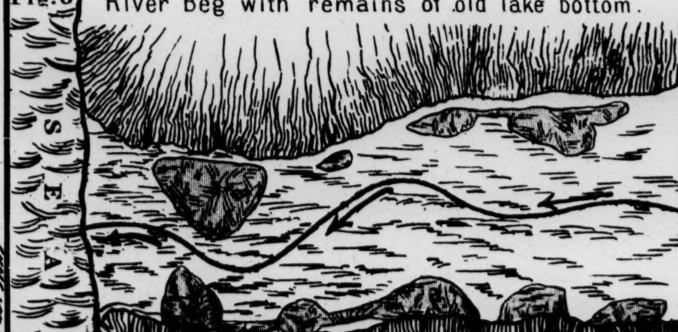


Fig. 6 River Beg with remains of old lake bottom.



His Grace the Duke of Argyll, at the meeting of the British Association in Glasgow in 1876, was pleased to allude in complimentary terms to the researches of the Committee, and to express a hope that a condensed abstract of all the boulders reported on, might ultimately be framed. The suggestion will receive the consideration of the Committee.

ARGYLLSHIRE.

1. *Glenelg.*—Blocks of grey and red granite occur in the drift-beds through which the river Elg has cut. The rocks of this district are not granite, but clay schists.

On the right side of the valley of the Elg, immediately above the road, about 2½ miles east of Glenelg, there is a grey granite boulder, 21 × 18 × 10 feet, as shown on figs. 1 and 2, Plate I. The sharp end points N.N.W. Its height above the road is 1020 feet, above the sea 1120 feet.

It goes by the name of the Macrae Boulder, in consequence of a prophecy by a Mackenzie of Kintail, that some day, when one of the clan Macrae is travelling on the road below, it will fall and crush him.

The boulder is on the very edge of a shelf of the hill, and projects beyond it about 6 feet, as shown in figs. 1 and 2, Plate I.

The rocks on which it lies are clay-stone schists. The boulder must therefore have been *brought* to its present position. It is said that on a hill some distance to the west there is a granite rock similar to that of the boulder. By what means, and how the boulder was deposited in its present precarious position, it is difficult to explain. Possibly, when deposited, there was no steep cliff, at the edge of which it now projects. The whole valley may have been filled with detritus up to the level of 1100 feet, and thereafter scooped out by the river, as the sea, in falling from one level to another, gave to the river more cutting power. This process of scooping might have continued for such length of time, that the cliff thereby formed at length reached the boulder.

2. *In Glen Rossdale* (about 8 miles from Glenelg), at a height of about 900 feet above the sea, there is a boulder of coarse red granite, 5 × 4½ × 3 feet, on the top of a narrow ridge of hypersthene rock, as shown on fig. 3, Plate II., on the left side of valley.

Its position also is precarious, and suggests a doubt whether, when brought here, it could have been deposited on the precise point where it now stands. There was nothing to indicate the direction from which the boulder had come.

3. There is another boulder on the right side of the valley, about 820 feet above the sea, 12 × 15 × 7 feet, fig. 4, Plate I.

It lies on a shelf near the ridge of a hill, and close to a slope of the hill which rises up from the boulder, and facing the N.W. The spot suggested the idea that the boulder had been brought from the N.W., and that this hill stopped its further progress. There is towards the N.W. an opening among the hills through which it might have been floated towards its present site.

4. A little lower down the valley (Rossdale), and on the same side, at a height of about 630 feet above the sea, there is a rocky knoll somewhat flat on the top, and presenting an area of about 8 or 9 yards in diameter, on which are five or six boulders lying pretty close together, as shown on fig. 5, Plate I. The boulders are granite, the knoll is mica schist.

5. At a still lower part of the glen there is a steep hill sloping down to the river. Near the top of this hill, and on the very edge overhanging the river, a boulder rests at a height of 300 feet above the river. The boulder is of granite, about 20 feet in diameter. It rests on mica slate rocks, which form a smooth surface sloping down towards the river at an angle of about 30°. Its position indicates transport from the north, as the land there is low enough to have allowed it to be floated over, whilst high hills to the south exclude that direction.

In the valleys where these boulders lie, there are some remarkable terraces. They were made known to the Convener by Captain Burke, R.E., two years ago, when he was still at the head of the Scotch Ordnance Survey. The surveyors employed in drawing the contour lines for the maps were struck by the horizontality and continuity of the terraces. Captain Burke was so obliging as to draw on the Convener's map lines to indicate their position. As these terraces suggest important views bearing on the position of the boulders, and their mode of transport, it seems not irrelevant to record the notes supplied by Captain Burke and give a copy of the map, to show where the terraces are.

Fig. 3

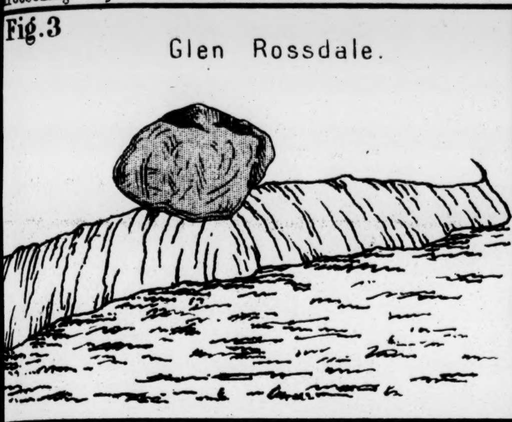


Fig. 13

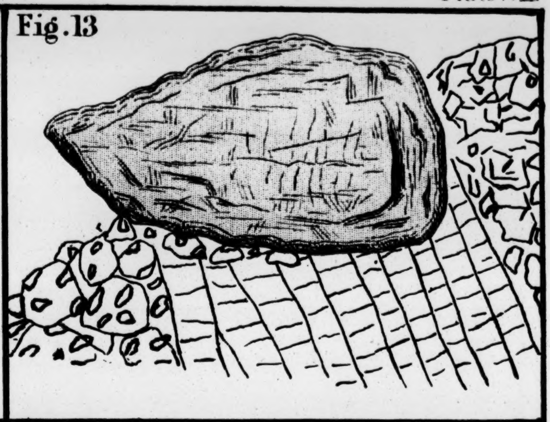


Fig. 14

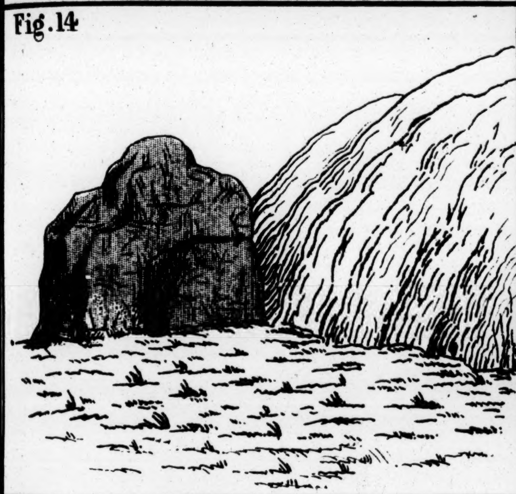
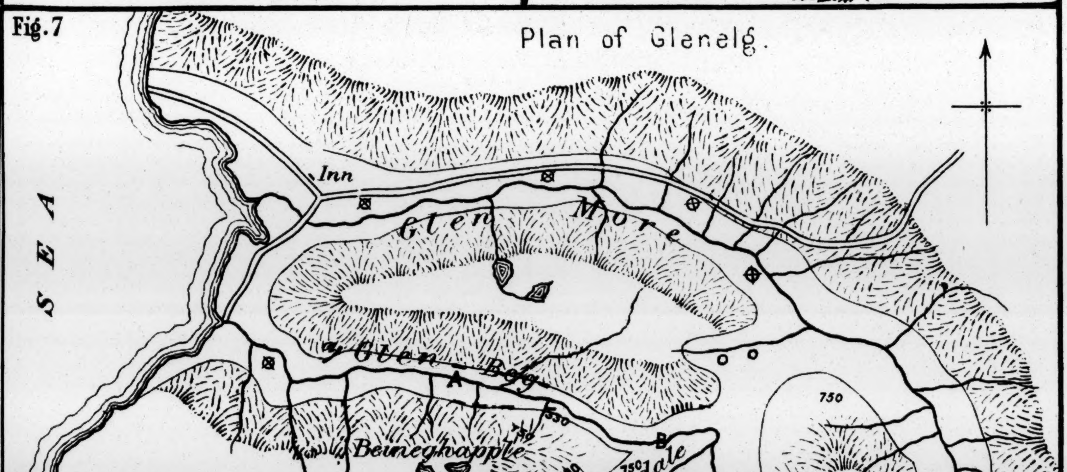


Fig. 15

Fig. 7



Before, however, describing these higher terraces, it may be right to refer to certain flats at the mouths of the valleys near the sea.

The town and village of Glenelg are situated on a flat which prevails all along the Scotch coasts, about 11 or 12 feet above high-water mark. Between Glenelg and Glenbeg the base of some high rocky cliffs is at the same level.

Mr Fraser, schoolmaster at Glenelg, having learned that the Convener was desirous of seeing examples of flat land, conducted him to the following spots:—

(1.) At Glenbernera, about half a mile to the north of Glenelg, there is a well-defined flat, about 44 or 45 feet above high water. A corresponding flat occurs at many other parts of the coast.

(2.) Behind and above the new schoolhouse at Glenelg, there is a considerable extent of flat land, at a height of 72 feet above high water. On the opposite, *i.e.*, the south side of the valley, which is half a mile distant, there is a flat at exactly the same height, judging by the spirit-level. The river has cut through this flat. Its original formation cannot be ascribed to river action.

Beyond the manse and church, there is another extensive flat, 88 feet above the sea.

In a higher part of the valley, there are terraces on a smaller scale. If they slope with the river, as they seem to do, *they* probably had been formed by the river, when it ran at a higher level, that is, when the sea also stood at a higher level than now.

Near the mouth of Glenbeg, about a mile from the sea, there is a great mass of detritus, through which the river Beg has cut its channel. There is a flat here also on each side of the river, the level of which is about 120 feet above high water.

Fig. 6, Plate I. is (from memory) a plan of this valley. The parts marked *a*, *a*, &c., are patches of detritus, the tops of which are all on the same level, or very nearly so, viz., 50 feet above the sea.

The whole valley apparently had been filled with detritus, when the sea stood at least 150 or 200 feet above its present level. As the sea retired, channels were cut in the detritus, not only by the main stream now occupying the valley, but by the numerous and rapid side streams from the steep mountains which enclose the valley on both sides.

At about 3 miles from the sea, the place in Glen Beg is reached,

where Captain Burke states he noticed a horizontal terrace on both sides of the river, at a height of 330 feet above the sea. It is marked (A) on plan, fig. 7, Plate II.

The Convener recognised a terrace on the right bank at 338 feet, but he could see none on the opposite or left bank. At a little distance farther up, there are on the left bank gravel knolls at a somewhat higher level. At this place, the channel of the river is about 40 feet below the terrace, and is of rock, which has of course prevented any deeper cutting of the drift beds.

At the junction between Glen Beg and Glen Rossdale (B) in the plan, there are very large knolls of detritus with flattened tops.

From the highest of these knolls, the Convener, on looking across the valley in a direction by compass E. by N., descried a terrace, continuous for about 80 yards, and apparently horizontal. Its position is indicated on the plan by five small vertical strokes. When the spirit-level was turned in a direction about E. by S., it struck on another horizontal terrace, about half a mile distant. All these flats are at one height, viz., 528 feet above the sea.

Higher up Glen Rossdale on the left bank, and at a spot about 1½ mile from (B), there is an extensive flat, which had been marked by Captain Burke. He states it at 750 feet above the sea. The Convener made it 760 feet. When a person is on the terrace, it is not distinctly traceable for more than 300 or 400 yards; but when viewed from the opposite side of the valley, at a distance of about a quarter of mile, it can be distinctly traced for more than a mile continuously; and at its east end it is seen to cross the ridge which divides Glen Beg from Glen Rossdale.

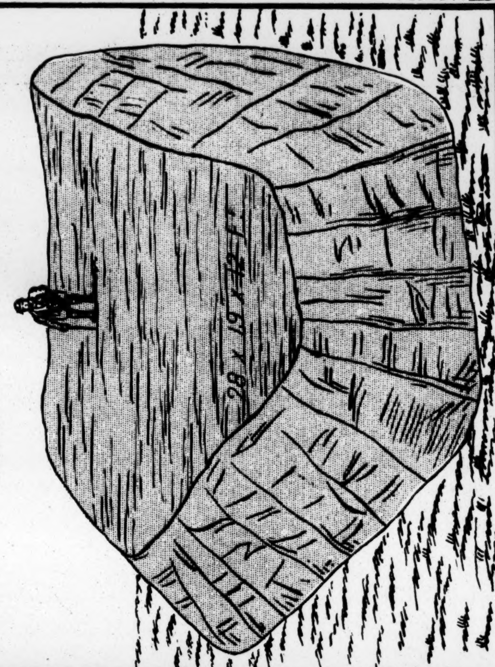
In a higher part of Glen Rossdale, and still on the left side of the glen, the Convener observed traces of a shelf at 853 feet, with a steep slope or bank below it of about 30 feet in height. The Ordnance surveyors observed traces of a horizontal terrace still higher, viz., at 1500 feet above the sea. The Convener, looking in that direction, observed, at a distance of about 3 miles, something like a horizontal line running for nearly a mile continuously at what might be about that height.

On the plan, Captain Burke indicates as existing in adjoining glens, traces of the 330 feet terrace by the cypher 0. These glens the Convener had not time to visit.

Fig. 10

Boulder, Kerrera, intercepted by rock & gravel cliff on S.E side.

Fig. 11

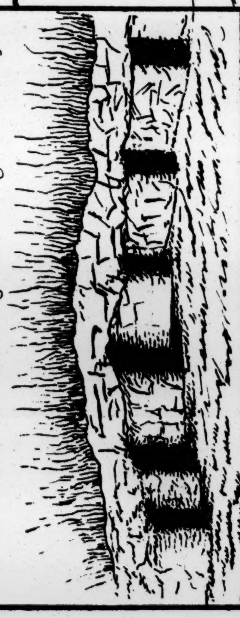


Boulder, Loch Lomond.

Fig. 9

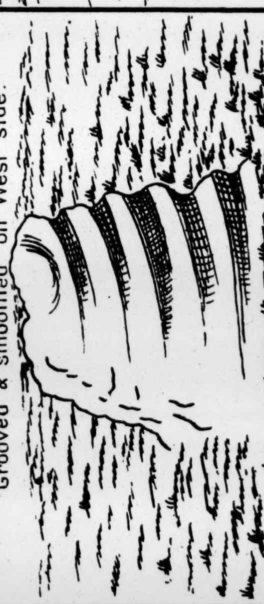
Boulder, Islay, intercepted by a hill.

Fig. 16



Joints in rocks, smoothed by agent from West. Joints facing East rough. (Loch Tay)

Fig. 17



Glen Dochart, Rock 1386 Ft above Sea. Grooved & smoothed on West side.

Fig. 12

Boulders lying on a Moor on West side of Loch Lomond

W. & A.K. Johnston, Lithog.

He has also put a X at the head of three several glens to indicate that at these places, and at a height of 750 feet, there are gravel heaps.

Some quotations may be made from Captain Burke's letter, dated 25th August 1876 :—

"I was up Glenelg yesterday. There is evidence of the sea having stood at more than one height, considerably above its present level.

"The only terrace of any consequence is in Glen Rossdale. It is about 300 yards in length. It has nothing in the least resembling Glen Roy.

"I will now answer your questions :—

1.—Height above the sea—

(1)	Principal terrace, about	750 feet.
(2)	Another, very doubtful, . .	520 ,,
(3)	Some rather more apparent, .	330 ,,

"This terrace, at the head of Glen Beg, affords strong evidence of a beach, such as now exists in all sea lochs hereabouts. Beds of gravel at 330 feet are cut through by the stream running through the valley.

"On crossing the high neck, 450 feet above the sea, and descending into Glenmore, similar beds were found at the head of that valley, at the same altitude.

"The spot marked in my sketch IIIII at 530 feet is very doubtful.

"The 750 feet terrace is visible in patches in Glenbeg and Glen Rossdale. I tried to trace it down Glenmore, for I have no doubt the land between these glens was once an island. But, although I fancied I found a mound sometimes, it can't be traced.

"The longest vestige of a terrace which I saw, is in Glen Rossdale, viz., about 300 yards; for the rest, there is only a mound here and there on the hill side.

"As to the width of the terraces, the greatest I saw is about 30 yards.

"You ask how high up these hills is sand and gravel found? I saw appearance of gravel at over 1200 feet. There are gravel heaps at 750 feet at the heads of the valleys marked X, at the spots one would expect to find them, and also at ⊕, apparently washed when the country was under water, and since cut through by the streams in the valleys. In fact, all the appearance is as if these valleys

were once sea lochs, just as are Lochs Nevis and Hourn at the present day. These marks are frequent throughout the Highlands. I have seen similar gravel-beds along streams in several other glens.

"Whether it will be decided to survey the shelf, I cannot say. But there is nothing definite except the bit in Glen Rossdale; and a surveyor would not find it easy, when on the hillside, to know what mounds he should show, unless he had previously run a contour at the required height.

"I have made two sketches, one of Glenbeg looking east, another of Glen Rossdale looking west, which I shall be happy to show you."

With reference to these last remarks by Captain Burke, it occurs to the Committee to express an opinion that, when the Ordnance surveyors discover on the hill sides terraces of the kind referred to, there should be some record given of them on the maps, accompanied by a contour line at the same level along the adjoining hills, so that it might be seen whether there are separate patches of gravel elsewhere at the same height. It is also desirable that when the officer at the head of the Survey verifies what the surveyors have found, and makes sketches of the terraces, these sketches should be given with the maps when published.

In walking down Glen Rossdale valley, on the right bank, the Convener fell in with a large mass of detritus, cut up into a series of knolls by the action of streams and rain. The height of these knolls above the sea was on an average 858 feet—agreeing pretty nearly with the level of the shelf already noticed as existing on the opposite side of the valley.

These remains of gravel in Glen Rossdale and the adjoining glens, looking to the height and the form in which they occur, seem conclusive as to the occupation of these valleys by the sea; and they confirm the inference derived from the position of the boulders, that the boulders were probably *floated* into these positions.

The Convener was at first puzzled to account for the circumstance that most of the large boulders which he saw in these valleys were not upon drift, but upon bare rock; and in many other parts of the country, the same thing occurs. If these boulders were floated by ice and thrown down, they must most generally have fallen upon the detrital beds then forming the sea-bottom, and not upon bare

rocks. When the sea retired, the boulders would then be on the drift, or buried in it. But when the streams from the hill sides began to flow and to remove the drift, the boulders would sink until rock was reached by them, where, of course, they would remain. The denudation of the old sea-bottoms has been everywhere so extensive, that very probably most of the boulders now existing are not in precisely the exact positions which they occupied when originally deposited.

6. *In the Pass of Brander*, where the River Awe flows out of the lake of the same name, there are several boulders deserving notice.

On the right bank of the river, near the spot where there is a pier for the small lake steamer, there are two terraces on drift. Both terraces have boulders on them. The boulders are of reddish granite. The rocks in the Pass are a slaty schistose rock like greywacke. The boulders have apparently come from some distant region, as the granite of the Loch Etive hills is not red, but almost entirely grey in colour.

The height of Loch Awe above the sea is (by Ordnance Survey) 110 feet. The lowest terrace is 68 feet, the higher terrace about 120 feet above the loch.

Both terraces appear to be horizontal. They can be traced for nearly a mile continuously.

On the opposite or left bank of the river no corresponding terraces are distinguishable. That side of the valley consists of nearly bare rock, and is almost vertical, so that there cannot be expected to be on it any trace of a beach line.

Have these terraces in the Pass of Brander been formed by a lake or by the sea? In a lower part of the valley there is a large amount of detritus, and it reaches at some places to a higher level than the terraces. The valley in that lower part is narrow, so that there might have been a blockage for a lake. On the other hand, how can the granite boulders be accounted for which are on the terraces? If, as seems most probable, they have come from the north, they must have been floated by ice on a current flowing from the N.N.W.

7. *Inveraray.*—His Grace the Duke of Argyll (Nov. 1876) conducted the Convener to a small hill, about 1000 feet above the sea,

at a place called "*Brae Leckan*," 7 miles west of Inveraray, well covered with angular boulders. The boulders were of the same nature as the rocks of the hill—a dark grey porphyry. But the boulders had evidently been transported to the hill from some other place, there being no cliff from which they could have fallen. The Duke thought they had been floated from the eastward, and in that direction certainly the land was lower than in any other direction. But the Convener observed that towards the west there was an opening among the hills low enough and wide enough for a current to have flowed to and over the hill on which the boulders rested.

8. A few miles to the *north of Inveraray* there are some huge boulders of a coarse conglomerate, quite distinct from any of the rocks in the immediate neighbourhood. The rock of these boulders is a greenish or grey coloured Silurian rock full of quartz pebbles. One of these conglomerate boulders, weighing about 60 tons, is on flat ground about 800 feet above the sea, and resting on gravel. Another, $10 \times 9\frac{1}{2} \times 7\frac{1}{2}$ feet (weighing about 48 tons), is on the left bank of the River Arey, and about 180 feet above the sea.

The gamekeeper, who pointed out these boulders, said that there was no rock of the kind composing them, except at a place about 6 miles due west. Between that spot and the sites of the boulders there were several ranges of hills and valleys.

When the subject was mentioned to the Duke of Argyll, he corroborated the gamekeeper's statement. He informed the Convener, that there is conglomerate rock, of the same character as that of the boulders, on the summit or ridge between Loch Awe and Loch Fine, which lies to the north-west of the boulders.

On the tops of several of the hills to the north-west of Inveraray, about 700 feet above the sea, boulders were noticed where it was manifest, from their peculiar position, that they could have got into it only by coming from the west. Sketches of these were taken.

9. *Oban and Neighbourhood.*—(1.) At Dunolly, close to the sea shore, there is a grey granite boulder $12 \times 8 \times 6$ feet. It is about 20 feet above high water, and rests on an old sea beach. Its longer diameter points W. by N.

The nearest rocks of grey granite are in Loch Etive, situated to the eastward. There are ranges of hills between Loch Etive and the

site of the boulder. Moreover, the boulder is close to the foot of a high rocky cliff, which being on the east side of the boulder, must have prevented the boulder reaching its site, except by transport from the westward,—probably the north-west, as the island of Kerrera is situated to the west and south-west, and would prevent the boulder coming from that direction.

The Convener was accompanied by a gentleman resident in the neighbourhood (Mr Clerk), well acquainted with the Loch Etive granite, who expressed doubts whether the granite of this boulder was of a similar composition.

(2.) The north part of Kerrera Island is strewed with numerous grey granite boulders, all well rounded. Most of them are on the beach, and on the old terraces adjoining the beach. There are some also, on Ballimore Farm, at heights of from 357 to 437 feet above the sea, on short terraces or flats of detritus facing the east and north-east.

In these cases there would be less obstruction to a transport from Loch Etive than in the case of the Dunolly boulder, but the range of hills near Glenlonan, reaching heights of from 500 to 1500 feet, still presents a difficulty.

If the Dunolly boulder came from a northern source, the Kerrera boulders probably came from the same quarter.

(3.) On the hills to the east and north-east of Oban, there are numerous boulders, chiefly of granite, whose position does not suggest one direction more than any other.

The rocks of these hills being clay slate, the boulders on them must have been transported from some distant quarter.

The granite is grey of different varieties, and very like the Loch Etive granite. But there are others, with large crystals of quartz and felspar, which betoken some other source.

One of this kind is on a hill to the south of the old public road between Oban and Loch Etive, at a height of 530 feet above the sea. It is extremely angular, and rests on a bare rock of the hill. This position would most easily have been obtained by floating ice.

Besides these *granite* boulders there are some of *dark porphyry* and of quartzite, which most probably come from the north.

This conclusion is somewhat strengthened by the circumstance that in this district, where the rocks are smoothed and striated, the surface of the rocks slopes down towards and faces the north, and

the striæ run north and south. Examples occur on the old public road before-mentioned.

(4.) An angular boulder of grey granite, $11\frac{1}{2} \times 7\frac{1}{2} \times 7$ feet, occurs at Inverlievern, on Loch Etive, above Bonawe Ferry. This boulder rests on three or four smaller granite boulders, and these again on bare granite rock. There is no hill from which it could have fallen. It must have been transported. A sketch was taken.

It rests on the 40 feet old sea-margin, which is visible round the greater part of Loch Etive.

(5.) The Convener was informed of two very large boulders in the district between Loch Etive and Glen Lonan, at places called Auchnacoshen and Duntarnichan. But he was unable to reach them.

10. *Fasnacloich on Loch Creran.*—Captain Bedford, R.N., wrote to the Convener, calling his attention to a large boulder which he had seen when surveying for the Admiralty. He sent a description of it, and mentioned that its average girth was 30 feet.

The Convener discovered the boulder. It had recently been blown up into four or five fragments, with a view to being used for building a bridge. But they were found unsuited for the purpose, being too hard for masons' tools. The rock consisted of a dark porphyry, with which the Convener was unacquainted.

Mr Hall, the tenant of the farm of Fasnacloich, on which Captain Bedford's boulder was situated, conducted the Convener to a spot, about a mile higher up the glen, where there were multitudes of much larger boulders of the same species of rock. The spot proved to be a mass of detritus, consisting of water-borne gravel, forming a sort of terrace abutting against the hill, which forms the north-east side of Glen Creran. This terrace is covered by numerous boulders, some of very large size. A view of one of them is given in fig. 8, Plate I.

This boulder has the Celtic name of "*Fas-na-cloich*," or "*Fas-na-clach*," which means "stone with growth,"—referring to three trees growing on it, two on the top being firs (each about 15 feet high),—one at the side, a stunted oak. The name of the farm occupied by Mr Hall, and of the residence of the proprietor, Captain Stewart, is Fasnacloich, so-called, most probably, after the boulder.

The boulder with the three trees on it is about 25 yards in girth; its length is 23 feet, its width 15, and its height, in so far as visible above ground, is 15 feet.

Another boulder, a few hundred yards to the south, measured (above ground) 18 × 18 × 12 feet.

It deserves notice that the sharpest end of each boulder points in the same direction—viz., about S.W. (magn.)—*i.e.*, towards the mouth of Loch Creran.

The terrace on which these boulders lie, is about 290 feet above the sea.

Hall mantioned that, in a higher part of Glen Creran, the boulders are more numerous, and some of them larger than the two examined.

All the boulders appeared to be of the same species of rock. Hall, who evidently had some practical knowledge of rocks, called it a black granite, and affirmed that there was no granite like it in all that district. The rocks of the mountain on the opposite, or south side, of Loch Creran, rising steeply to a height of above 2000 feet, he knew were a grey granite. The Loch Etive granite, about four miles to the south, and the Durra granite, about eight miles to the east, being of a light grey colour, he had always wondered where these dark coloured boulders could have come from.

The rocks in Loch Creran, and in the hills immediately adjoining, are a blue schistose clay slate, with a rapid dip.

One or two other points may here be noted, communicated by Hall :—

A small river runs into Loch Creran, at its head, flowing out of a small fresh-water lake, which is separated from the sea by a spit of gravel and sand, crossing the valley, and cut through by the river. The sand, Hall stated, is full of sea-shells, and so is the bed of the lake, and even the channel of the river before reaching the lake. In this last-mentioned river, the shells are in a bed of fine clay—whitish in colour, which is used as a manure for arable land. In fact, it is this bed of shell clay which originated the name "*Crer-an*," i.e., "*Clay*," or "*Chalk River*."

These facts indicate, of course, a period when the sea stood at a higher level—to the extent of at least 20 feet, which is about the height of the shelly bed above mentioned. When the sea fell to its present level, a blockage of drift, now between the sea and the lake, caused the lake to be formed, with an overflow by the river, which runs out of the lake into the sea. There are several

other places in the West Highlands where there are fresh-water lakes close to the sea, formed in like manner.

With regard to the boulders, it occurred to the Convener, judging from their locality and their position, that they had probably been floated up Loch Creran, and been then stopped in their further progress by the contraction of the valley and the higher level of the land.

But if they were floated up Loch Creran, from what quarter did they come? It was natural to look to places facing the mouth of Loch Creran, if in these places there were mountains composed of rocks similar in composition to the boulders. The island of Mull, situated to the W.S.W. of Loch Creran, seemed therefore to be one locality which might have supplied the boulders, as from Mr Judd's instructive paper on Mull,* describing numerous varieties of granite in the mountains of that island, it appeared likely that rocks of the same character as the Fasnacloich boulders existed there. With the view of testing this idea, the Convener sent specimens of the boulders to Professor Judd, who he heard had, during the past autumn, spent three months among the Mull mountains, and asked him to state whether he recognised the rock composing these boulders as being identical with, or at all events similar to, any of the Mull rocks? Professor Judd was so obliging as to respond to the application.

With the Fasnacloich specimens, there went to Professor Judd specimens of the rock composing two very large boulders on the shore at Appin, which rock the Convener found on examination to be the same as that of the Fasnacloich boulders. These Appin boulders lie on upturned blue clay slate rocks. Their shape indicated that they had undergone great friction, in consequence probably of being rolled over the sea-bottom by icebergs floating through what was then a sea strait, but now the Linnhe Loch, and the chain of lakes forming the Caledonian Canal. Sketches of these Appin boulders were taken. The largest is 15 × 11 × 10 feet. Both boulders are well rounded at the angles.

Professor Judd's Report is in the following terms:—

"*Appin Boulders, No.* 1.—This rock is not a granite, but a rock of basic composition. It appears to be a gabbro with some black mica. It is very similar in character to the gabbro of Skye, Rum, Ardna-

* See "Geolog. Society's Trans."

murchan, and Mull, which are described in my paper. I think there is no room to doubt it was derived from one of these localities —the rock is so peculiar and well characterised.

"*Fasnacloich Boulders*, Nos. 2, 3, 4, are very ordinary gabbros, such as form great mountain masses in Skye, Ardnamurchan, and Mull. These rocks are of a striking character, and differ from any which I know of on the mainland. I think it is certain, they were derived from the Western Isles."

Professor Judd, in his paper on the ancient volcanoes of Mull, Skye, and Ardnamurchan, refers to proofs that these volcanoes reached a greater height above the sea-level than any of the existing Scotch mountains, perhaps even to the height of 14,500 feet,* and that "denudation" had acted to an enormous extent in breaking up the old volcanic rocks and lowering their height. Professor Judd does not particularly specify the nature of the denuding agent which he supposed produced this effect. But if the sea with ice floating in it, at a height of say 2000 or 3000 feet above the present level, be allowed to be a denuding agent, it is easy to see how the boulders of Appin and Fasnacloich, if derived from Mull or Ardnamurchan, might have reached their present positions.

The distance of Appin and Fasnacloich from Mull and Ardnamurchan is about 30 miles. The intervening sea has in some places a depth of 100 fathoms. The island of Lismore, which is in this part of the Linnhe Loch, at one spot only reaches a height of 420 feet. A sea current flowing across Mull and Ardnamurchan, towards and through what is now known as "Glen na Albin," with mountains on each side of the Glen reaching to 2000 feet above the present sea-level, might, by floating ice, have carried boulders and lodged them in lateral valleys, such as Loch Creran.

11. *Crinan Canal.*—At the summit level, about half-way between the two extremes, there is a large accumulation of boulders, chiefly angular in shape. On the west side of the canal at the "*locks*," a body of rock stands up, whose surfaces facing the north present marks of abrasion as if caused by some body or bodies passing over from the north. On the south side of this rocky knoll, lie a number of boulders which, if they came from the north on floating ice, may have been projected over the knoll by its intercepting the ice

* Quarterly Journal of the Geological Society for August 1874, page 259.

in its farther progress through this kyle or sea channel. One of the largest of the boulders is lying with its longer axis N. and S., or parallel with the general axis of the valley at this point. These conditions would be met by the sea standing at a height of from 140 to 150 feet above the present level. On both sides of the valley here there are horizontal lines traceable at that height, as if made by the sea.

12. *Island of Islay.*—The Convener, in August 1877, paid a visit to this island, for the purpose chiefly of examining the famed raised sea beaches on the adjoining island of Jura, and also of inspecting some boulders of which notice had been sent to him.

(1.) On the farm of Lossit, about three miles south of Port Askaig, there are four or five boulders of large size. Only two were seen.

One of these, 13 × 8 × 8 feet, is a composite rock containing crystals of quartz, augite, and hornblende. The stone is extremely hard; it was with much difficulty that a small specimen was detached. The boulder was resting on a bed of bright yellow clay, apparently a sediment of deep water. The rocks of the district are a slaty schist. On inquiry, it was surmised that rock of a similar kind existed near Kildalton, about 20 miles to the S.E. But doubt exists on this point.

The other boulder, scarcely so large as the foregoing, resembled a compact Silurian rock, containing numerous crystals of a whitish felspar.

There was nothing to indicate how or from what quarter these boulders came. Their height above the sea was about 300 feet.

(2.) On the farm of Arnahoo, about three miles north of Port Askaig, and 228 feet above the sea, a boulder stands conspicuously on the summit of a hill in a position most precarious (fig. 8, Plate III.). The rock composing the boulder is a hard porphyry, quite different from the rocks of the hill on which it rests. Its height above the sea is 228 feet, and the hill itself is about 300 yards from the sea, towards which it slopes very steeply.

The boulder is not absolutely on the highest peak of the hill, but a few feet below the peak, and on the slope which faces north by east (magn.). The only way in which the boulder could have stuck on this slope was by its coming right against it, and being let down on it gently, *i.e.*, without falling from a height. It

must have come in a direction from N. by E. If floating ice brought it—and no other way is here conceivable—from the south, the boulder could not have reached its present position. It would have stuck on the south side of the hill. It could not have reached its position by a somersault over the hill top, for the impetus acquired by its fall would have projected it down the hill altogether.

As bearing on the direction from which this boulder may have come, it is proper to add that towards the north-west there is a range of hills, apparently much higher than 300 feet, whilst towards the north and north-east it is open sea, and the island of Mull is in that direction.

(3.) On the farm of Persibus (occupied by Mr Rounsfell), about three miles S.W. of Port Askaig, four or five boulders, well rounded, occur, and were seen. They are all of a hard porphyritic rock, differing from any of the Islay rocks. Their height above the sea was found to be about 228 feet.

With regard to the probable line of transport to their positions, it may be noticed that towards N. by E. there is an opening or depressed part of the country, through which the boulders might have been floated to their sites.

Mr Rounsfell pointed out a very large boulder situated on a hill slope to the north, about two miles distant, which, however, the Convener was unable to visit. But Mr Ballingall, factor on the Islay estate, has had the kindness to examine the boulder, at the request of the Convener, and he reports as follows:—"Girth, 33½ feet; height, 11 feet; length, 12 feet; breadth, 18 feet. It lies on clay slate rocks, and is all exposed to view. Its *thickest* end faces S.W. Its height above the sea is 410 feet." Mr Ballingall has sent with his letter a small chip of the boulder. It proves to be an igneous rock, with much hornblende. It has probably come from some northern region. The weight of the boulder Mr Ballingal estimates at 25 tons.

(4.) On the south side of the high road between Bridgend and Port Helen a boulder rests at the foot of a low hill which faces about due north. The boulder is tolerably well rounded, and about 7 feet in diameter. It is a stranger to this district. Most probably it came from the north like the rest, and was in its farther progress intercepted by the hill at the base of which it lies. Its height above the sea is about 50 feet. (See fig. 10, Plate III.)

(5.) On the west coast of Islay, in the parish of Kilcheran,

there are porphyritic boulders lying on the blue slate rocks, and so situated as to make it clear, that they have been brought and lodged there by some agency from the N.W.

Below the old parish church of Kilcheran a small stream joins the sea through a valley in a direction W.N.W. (magn.). The rocks on the south bank of the stream are ground down and striated in such a way as to show that some force has passed obliquely across the valley from N.W.

In regard to these Islay boulders, it is very apparent that they have all come from the north—some of them very probably from Mull. It is also rather remarkable that the largest should occupy sites very nearly on the same level, viz., 228 feet above the sea, a circumstance suggesting the same means of transport. As bearing on this last point, it may be observed, that on various parts of the Scotch coasts there are traces of old sea-beaches, at heights between 250 and 500 feet above the present sea-level.

13. On the *Peninsula situated between the Firth of Clyde* (*on the east side*), *and Loch Striven* (*on the west side*), there are several boulders of some interest.

(1.) At Dunoon and Kirn there are boulders of a micaceous sandstone rock, all well rounded, lying on the edges of the blue slate rocks which form the beach. One has had painted on it the words "*Jim Crow*," being $15 \times 8 \times 6$ feet; another, the words "*John Bull*," $15 \times 12 \times 6$ feet.

It was stated to the Convener by a local correspondent, that rock of the same nature as in these boulders occurs in the Holy Loch, situated about a mile to the north-west.

Two of the boulders on this part of the beach are so fixed as to indicate from what quarter they must have come into their present position, viz., from the North. Sketches of these were taken.

(2.) Along the shore towards Innellan there are numerous boulders differing from the rocks on which they lie. Some of these rocks show surfaces smoothed and striated, the striæ running north-east and south-west—a direction parallel with the general line of coast. Some local agency has, therefore, probably been at work here.

(3.) On the east shore of Loch Striven lies the large, well-rounded boulder, called "*Craig na Calleach*," or "*Stone of the Witch*"—the legend being that in former times, the witches inhabiting both

banks of the loch, threw these great stones at one another. It is said that on the west bank of the loch, near Strome Point, and almost immediately opposite to "Craig na Calleach," there is a boulder of about the same size—which, however, the Convener was unable to go in search of. "Craig na Calleach" is a compact schist of a light grey colour, with thick nodules of quartz in it. The rocks of the beach on which the boulder lies are a slaty schist of a greenish blue colour. A sketch was taken.

(4.) On the farm of *Ach-na-foud*, situated about a mile from "Craig na Calleach," there is on the slope of a hill an angular boulder. It is at a height of 222 feet above the sea, and on the edge or verge of a precipitous bank. It rests partly on rock, and is in a very critical position. If the bank be now in the same condition as when the boulder was deposited, it must have been let down very gently or gradually, to avoid receiving an impetus which would have caused it to roll down the bank. A sketch was taken.

BERWICKSHIRE.

In the parish of Dunse the boulder clay has lately been cut through for some hundred yards to make a new road, and to a depth of 8 or 10 feet below the surface of the boulder clay. Beds of gravel and sand lie over the boulder clay, in some places to the thickness of 12 feet. On an inspection by the Convener (November 1877), in company with Mr Stevenson, Dunse, boulders in the clay were recognised as having all come from the west, chiefly W. by N. The Kyles Hill and Dirrington Hills porphyries were among these; the former is three miles W. by N., the latter six miles W.N.W. There were also sandstones with fossils, which Mr Stevenson knew to have come from a sandstone rock a few hundred yards to the westward, and which he pointed out to the Convener. The fossils were the ordinary plants of the coal formation, and an annelid.

DUMBARTONSHIRE.

1. *Loch Lomond.*—The large Mica schist boulder reported to the Committee by Mr Jack, and mentioned in the Committee's Second Report (Roy. Soc. Proc. for 1872-73, p. 152), was visited by the Convener, in company with Mr Smollett of Cameron House. Its

provincial name is "*Kerstone Galloch*," it is situated on the farm of Callendoon, and is about 150 feet above the sea. Its length is 28 feet; width 19 feet; depth 12 feet.

It is shown on fig. 11, Plate III., with Mr M'Arthur, tenant of the farm, standing on it.

Originally, the boulder had been in a somewhat higher position. A small stream running past the boulder at its east side had washed away part of the gravel bed on which it had been resting,—so allowing it to sink.

With reference to the quarter from which this boulder was transported, Mr Jack suggested that if it came from the west, it must have come over hills from 1000 to 1200 feet high; and therefore he thought it more probable that it had been floated south down the valley now occupied by Loch Lomond, and then floated west up Glen Fruin.

It appeared to the Convener, that the line of transport was more likely from the westward. The land towards W. by N. (true), is on about the same level as the land to the north-east, as shown by the contour lines on the Ordnance maps of the district. If the boulder came from the westward, there would be no obstruction to its progress in a direct line; whereas, if it came from the north end of Loch Lomond valley, as suggested by Mr Jack, it must have changed its course to reach Callendoon.

2. On a moor, about half a mile to the north east of the above boulder, there are several smaller boulders of mica schist, of the shapes and sizes shown on fig. 12, Plate III.

It will be observed that they all occupy similar positions in respect of their longer axis, and their sharpest end. Their height above the sea is about 250 feet. The rocks of this district are Old Red Sandstone. There is much probability that these boulders had been left here by floating ice, in a current flowing from the westward; and that they acquired their bearings from the action of the current.

3. On the west side of Loch Lomond there is at Arden a low valley, which runs up from the Loch in a westerly direction. The summit level of this valley towards the west is about 150 feet above the sea.

Along the south side of this valley a number of boulders, chiefly of primitive rocks, have been deposited. They are at a height of about 94 feet above the sea. As usual, the most frequent position

is here, as elsewhere, N.W. and S.E. for the longer axis, and the sharpest end towards the west.

4. In the policy of Cameron House a boulder of gneiss, $6\frac{1}{2} \times 5 \times 5$ feet, is lying on gravel, and at a height of about 55 feet above the lake, or 80 feet above the sea. Its longer axis is N.W. and S.E.

5. There is a hill called "Caer-man," about 3 miles to the S.W. of the south end of Loch Lomond. Its height above the sea is 720 feet. From its top, a good view is obtained of Helensburgh and Greenock towards the S.W.

The rocks on the top of this hill are a coarse porphyry. Huge fragments have been strewed in great abundance down the side of the hill sloping eastward, and especially S.E. The unmoved rocks present their west surfaces rounded and smooth, their east surfaces angular and rough.

On examining the separate blocks where heaped upon one another, it was apparent that the uppermost blocks, to obtain their positions, must have been projected on the others from the westward.

EAST LOTHIAN.

Linton.—On the farm of Drylaw, a greenstone boulder, $5\frac{1}{2} \times 3\frac{1}{2} \times 3$ feet, was found in cutting a deep trench through the boulder clay. The N.W. end was the most rounded. The longer diameter was N.N.W. (magn). There were no striæ on the top, but there were horizontal striæ on the two sides fronting the N.E. and the S.W. These two sides met in an angle towards the N.W. If a current had flowed from W. by N. the current would divide at the angle; and if ice floated in the current, the striæ on the two sides might have been produced by hard pebbles from the westward pushed against them. The smoothing and striation on the north were greater than on the south side. Close to the boulder there were pebbles of limestone shale, sandstone, and coal, which most probably came from the westward. The nearest greenstone rocks are on the Garlton hills, situated about 6 miles to W. by N. The boulder, therefore, most probably came from these hills.

About half a mile to south, there are rocks (viz., in Linton village, and in a railway cutting to the west), presenting smoothings and striations, made by some agent moving over them from W. by N.

FIFE.

1. *Isle of May.*—There are small Sienitic boulders on west side, at sea-level. On the west side there are also smoothed rocks. Direction of smoothing agent has been from W. ½ N. No boulders or smoothings are on east side.

2. *In Bogward Den* (Mr White Melville's property), 3 miles west of St Andrews, there is a boulder of conglomerate rock. Probably it came from Drum Carro Craig, which is said to be same species of rock, and situated some miles to N.W. The legend is, that the devil threw it from that hill, when the first Protestant church was being erected at St Andrews.

3. *At Kincraig, Fife*, there is on the beach a granite boulder, with girth of 23 feet and height of 4 feet. The lower half is angular, the upper half rounded. Has this boulder been floated from westward, and been stranded on the rocks at Kincraig? Stirling Castle, which is visible, bears W. ½ N. But it probably came from a more north-westerly direction. Fragments of this Kincraig rock (a trap tuff), have been carried eastward, and were found in the cuttings made for the railway 2 miles distant from Kincraig point.

4. *At Elie.*—Whinstone boulder on beach, 8 × 4 × 2½ feet. Its longer axis N.W. Striæ on boulder run N.W.

INVERNESS.

The Convener having been informed by the officers of the Ordnance Survey that some remarkable horizontal terraces had been discovered by them in Glendoe, a valley branching off from Glen Morriston, on the north side of the Caledonian Canal, he took the opportunity, when paying a visit to Mr Ellice of Invergarry, of going to Glendoe.

Under the guidance of two gamekeepers on the property of Mr Grant of Invermorrison, who reside at the foot of Glendoe, the Convener proceeded to the head of Glendoe, the place indicated in his map by the Ordnance Surveyors.

Unfortunately, a heavy fall of snow had (17th October 1877) occurred during the night preceding this visit, and it continued during the expedition.

There was at first some difficulty in making the gamekeepers comprehend the spot wished to be reached; and it was not till the party had gone some miles up the valley of the Doe, that the gamekeepers began to see what was sought for at the head of the glen. This was brought about by the Convener drawing attention, as he proceeded up the valley, to two lines of a terrace or flat noticed by him on a hill on the opposite side of the river. On his asking the keepers, whether there were similar lines at the head of the glen,—still about 6 miles distant, as they alleged,—the answer was, that there were such terraces, and so remarkable, that on one occasion, when accompanying a shooting party, some of the gentlemen remarked, that marks were there of Noah's flood!

The Convener was encouraged by this information, and in spite of snow and wind continued his progress up the glen.

The keepers stated that the marks to which they referred were on "English Hill;" and that though this hill was rocky on some parts, there was a great deal of sand and gravel near the top.

Following up the river Doe, a point was reached where the river divided into two branches, and called by a Celtic word meaning "Tongue of the Burus." The portion of the stream towards the right has the name of "Carriscreuch," or "Middle Corry;" and it was along that stream, flowing through what the keepers called "The Long Glen," that "English Hill" could best be reached. But the snow was here so deep, that no track was visible, and walking became dangerous, at least to a stranger.

At this point a consultation was held. The height above the sea reached was only about 850 feet, whereas the highest terrace marked by the Ordnance Surveyors was 1280 feet, and apparently still about 2 miles further up the glen.

The gamekeepers' advice was to abandon any hope of reaching the terrace, and to be satisfied with a distant view of the place, which could be obtained from a low hill in front.

This low hill accordingly was ascended, and with satisfactory results. The hill itself was found to consist, as shown by numerous scaurs, of fine gravel and sand; and on its flat top, the aneroid showed a height above the sea of 1190 feet.

This gravel knoll was as it were in an amphitheatre of hills, on several of which, towards the west, horizontal terraces were observed,

at a somewhat lower level. These appeared to run continuously for about a mile. On the opposite side, the hill bearing about east showed a short line at the same level. Looking towards "English Hill" on the N.E. no terraces were discernible; the snow, owing to the direction of the wind (which was west), was so thick on the slope of the hill facing the knoll, that inequalities, if any existed, were undiscoverable. But one of the keepers pointed in the direction of the part of the hill for the terraces he had before spoken of. The part so pointed out seemed to be about 2 miles distant, and at an elevation of about 100 feet above the knoll on which the party were then standing.

The Ordnance Surveyors had marked on the Convener's map two lines of terrace, one at 1280 and the other at 1140, as existing on a hill on the left side of the Doe water. Though, from the circumstances above stated, it was impossible to make out these terraces, there was enough discovered to show a line at the lowest of these levels on the other hills adjoining—and the existence of detritus quite capable of being formed into a terrace at a much higher level. One of the keepers stated that on a hill towards the N.W. there were beds of gravel and sand to the very top, and without any covering of turf. He pointed in the direction of Ben Doe, which has an elevation of 2000 feet above the sea.

The Convener having on his way up Glen Doe observed several large boulders on the slope of a hill above him on the left hand, resolved to visit them on his way back. So, accompanied by one of the keepers, he ascended the hill, and in looking across the valley, he discovered four horizontal terraces on the opposite hill, and continuous for about half a mile. They were apparently on detritus, for at one spot, where a rock projected, there was an interruption.

The uppermost terrace the aneroid showed to be about 985 feet above the sea, the lowest about 895.

The first of the boulders visited was 919 feet above the sea. Its dimensions, roughly measured, were $14\frac{1}{2} \times 11\frac{1}{2} \times 7$ feet. It was a coarse reddish granite, and very angular. It could not have been rolled or pushed. It seemed to bave been carried from its parent rock, wherever that was, without undergoing any change of form. It was resting on gravel and sand.

The next boulder reached was at a height of 1204 feet above the

sea. It also was a coarse red granite. It was about 30 yards in girth, and 14 or 15 feet in height. It is known as "The Glen Morrison Stone," probably because of being the largest boulder in the glen.

This boulder is on a flat, and in looking across the valley, a terrace is seen which corresponds in level with the boulder.

In several parts of the hill the boulders were in clusters or groups, piled over one another.

It deserves notice that all these boulders were resting on gravel and sand; and that the hills on both sides of the valley were thickly covered with detritus.

The gamekeepers spoke of a very large boulder at Clachnaharry, about 16 feet high, on the south side of Loch Clunie, which is two or three miles to the west of Glen Doe. The Convener saw it through his glass.

It may be added here, that Mr Ellice of Invergarry informed the Convener of a large bed of pure white sand, which he could not distinguish from sea sand, existing on a property belonging to him in that district, at a height of about 1000 feet above the sea.

MID-LOTHIAN.

1. In September 1877 the Convener visited excavations on the *north side of Craiglockhart Hill*, about two miles S.W. of Edinburgh. His attention was drawn to them by Mr Hutchison of Carlowrie.

These excavations were in the boulder clay. A number of boulders had been exposed, and were still undisturbed in their original positions.

The largest was angular, the smaller boulders were comparatively round. The greatest number were of blue whinstone rock. Among the smaller boulders, there were some of sandstone. The contractor for a large new building about to be erected being present, had his attention drawn to the sandstone boulders, and was asked if he knew any rock of the same kind which was in sufficient quantity to be quarried? He said that the sandstone of Hailes Quarry and Redhall Quarry was the same rock as that of the boulders. On being asked to point out the direction of these quarries from where the boulder lay, he pointed in a direction which was N.W. (by compass).*

* These quarries are about a mile distant from the site of the boulders.

The height of these boulders above the sea is about 340 feet.

2. At *Granton Harbour* (on west side) a very large blue whinstone boulder lies on the beach at high-water mark, part of which only is visible, the rest being covered and concealed by the sea wall which protects the road. On the upper surface of this boulder, there are innumerable striæ, the direction of which is W. 3° S. (magn).

About 100 yards to the eastward, there is another whinstone boulder having an iron ring in it, by which boats or vessels may be moored. There are striæ on it running in the same direction.

Between these two boulders there are some strata of hard sandstone rock, portions of which have been ground down and show striations running also as above.

3. In the *New Docks, situated to the eastward of Leith*, there is an immense bed of boulder clay, which continues along the coast eastwards for some miles.

This boulder clay at the Docks is covered by a muddy sand in horizontal beds about 8 or 10 feet thick. On the surface of the boulder clay there is a bed of oyster shells, of large size. There is as usual on the surface of the boulder clay a great accumulation of boulders, these having remained when the upper portion of the boulder clay bed was washed away by the sea. Most of the boulders are well rounded. The largest I saw, a light coloured blue whinstone, measured 10 × 8 × 6 feet, and was estimated to weigh 18 tons. About nine-tenths of the boulders are whinstones, but there are also some of quartz, limestone, sandstone, silurian, granite, and black ironstone concretions from beds of shale. These boulders have evidently come from the westward. On a great many, there are ruts or striæ all maintaining the same direction, viz., W. by N. (magn.) Those which are longer than they are broad, have their longer axis in the same direction.

Among the boulders, there were two metallic in composition, which deserve special notice.*

One, nearly spherical, measures 7½ inches in circumference, and

* The Committee have to thank Mr Hugh Campbell, who is professionally engaged in the formation of these new docks, for bringing to them the two remarkable balls here referred to, as well as for affording to the Convener opportunities for seeing the excavations.

weighs 24 oz. It was found about $4\frac{1}{2}$ feet down in the boulder clay, among the large boulders.

The other ball was even more spherical, its least girth being 30 inches, and its greatest 31 inches. Its weight was 54 lbs. It was found 10 feet below the top of the boulder clay.

Professor Crum Brown (Edinburgh University) was so obliging as to examine both of these balls for specific gravity and composition. He reports that the smallest ball is marcasite or white iron pyrites, and that its specific gravity is 4·63. It is entirely of pure ore, being apparently unmixed with any other substance.

With regard to the larger ball, the Professor has sent the following report:—

"The fragment of the large round stone which I took for examination had a specific gravity of 3·36. It consisted of a mixture of silica (not obviously crystalline) and iron pyrites, in the following proportions:—

$$\left.\begin{array}{ll}\text{"Silica,} & 52{\cdot}3 \\ \text{"Pyrites,} & 47{\cdot}7\end{array}\right\}\text{per cent.}$$

"Calculating from these numbers and the sp. gr., it is plain that the pyrites must be in the 'marcasite' form, as 'pyrite' would give a considerably higher sp. gr.

"The sp. gr. of the whole stone, *i.e.*, the mean sp. gr., was found to be 3·28. It cannot, therefore, be a uniform mixture."

Mr Murray having kindly offered to examine, microscopically, this large stone ball, has sent the following report:—

"*Challenger Office, Teviot Row,*
May 1878.

"Dear Sir,—The microscopic section of the boulder is made up of crystalline particles of quartz and marcasite. The marcasite fills the interstices between the grains of quartz; and among the quartz there are pieces of mica. (Signed) John Murray."

The Convener paid two visits to the excavations in the boulder clay at Leith to examine the spot where these two remarkable balls were found. He saw the superintendent, who was directing the excavations, and also the "*navvy*" who found the larger ball. The latter pointed to a whinstone boulder, and said the "big bullet" was close to this boulder.

There can be no doubt that both balls had come with the boulders, and had been deposited with them in the great bed of clay which covers the rocks in this district. This bed extends for fully half a mile on each side of the Water of Leith at its mouth, and reaches to a depth in some places of nearly 100 feet.

The black ironstone concretions found in this boulder clay bed show marks of friction. There are strata of shale containing such concretions, two or three miles to the westward. These concretions, as well as the boulders of granite and quartz, clearly indicate transport on a large scale from the westward.

The Convener learns from Mr Robertson, C.E., Albany Street, Edinburgh (who planned both the Albert Docks at Leith, executed some years ago, and the new docks now being constructed), that similar metallic balls were found in the Albert Docks excavations. But he has no specimens of them.*

* Since the foregoing was written, the Convener has received from Mr Charles W. Peach, of 30 Haddington Place, Edinburgh, a letter regarding marcasite nodules, from which letter, with Mr Peach's permission, the following extracts are made :—

"In the Falkirk and Slamannan district a band of these nodules, known as '*Speckled Ball Ironstones*,' occurs. It occupies a horizon a few fathoms above that of the '*Slaty Band Ironstone*,' the base of the Coal measures.

"The direction of the *striæ* on the rocks and the carry of the boulders and boulder clay is towards the east, and varies from E. 10° N. to E. 15° N.

"Near Kilsyth, and about 2 miles to the west of that place, the tributaries of the Corrie burn cross an area of blue shales, with several courses of ironstone nodules. Some of these are of iron pyrites (marcasite), and are known among the mining population as '*brassy balls*.' They occupy a horizon between the Hosie and Hurlet limestones, near the base of the carboniferous limestone series.

"The direction of the *striæ* and carry of the boulders in this district is E. or E. 5° N.

"Either of these sources could supply *balls* at Leith, as they are right in the direction of the ice-flow.

"As to *concretionary balls in sandstone*,—there is on the coast of East Lothian near Cockburnspath, to the north of Cove, a cliff of calciferous sandstone full of spheroidal concretions, which weather out on the wasting of the cliff by the sea, and being harder than the matrix, they lie piled up in great numbers at the base of the cliff. Many of them are of very large size.

"Similar concretionary balls occur in sandstone rocks at Grange Quarry near Burntisland, from whence, no doubt, the ball found lately at Leith was carried. (Signed) "C. W. PEACH."

This information in regard to marcasite brassy balls, the Committee deems highly interesting. If the marcasite ball found in the boulder clay at Leith, was transported from any part of the district situated to the north

4. At *Alnwick Hill, near Liberton Church*, at an elevation of about 350 feet above the sea, extensive excavations have been made in the boulder clay for the new Edinburgh water-works. The boulders consist chiefly of fragments of rocks, which are known to be *in situ* situated in districts of the country to the west and north-west. The great majority of the boulders are of hard red sandstone rock, such as occurs at Grange and Merchiston, to the west of Edinburgh, though these places are at a lower level. There are boulders of marine limestone, similar to rocks of that description in Linlithgowshire. There is an immense quantity of blue-coloured greenstones and dark-coloured basalts, and also buff-coloured felspathic rocks. There are some small boulders of pure quartz, which probably hail from the Silurian rocks to the north-west of Callendar and Doune.

Many of the boulders occupy positions, present shapes, and bear marks of some interest.

The largest seen by the Convener were about 7 feet long by 4 feet wide, and 2½ thick or deep.

The boulders were all well rounded and smooth, but more particularly so on what had been the upper and the under sides.

Mr Black, the superintendent of the excavations, being aware of the interest attaching to the position of the boulders and the *striæ* on them, had, with a compass, ascertained that the long-shaped boulders, before being moved, generally were lying in directions varying between W.N.W. and N.W.; that the striæ, when such existed, were almost always parallel with the longer axis of the boulder; and that there were striæ, sometimes only on the upper side, sometimes only on the lower side, sometimes on both sides. In one of the boulders, well rutted on the under side, he had remarked that the ruts were deepest at the east end of the boulder, and that they gradually diminished in depth and numbers towards the west. This feature might be accounted for on the supposition that the boulder, whilst being pushed forward, encountered hard obstacles which produced deep ruts on the boulder when the first

of Glasgow, as suggested by Mr Peach, what was the transporting agent to suit those localities? A glacier moving from west to east by the action of gravity would be hardly conceivable. The levels preclude that agent. A sea current, loaded with floating ice, seems a more probable conjecture.

contact took place, afterwards the boulder would rise over these obstacles, and in consequence the striæ produced by them would diminish in depth.

5. *Tynecastle, near Edinburgh.*—A basalt boulder, $4\frac{1}{2} \times 4 \times 2$ feet, was discovered, striated on both upper and under side, but the ruts were much deeper on the under side. The under side ruts had begun to be formed at the east end of boulder,—the striæ on the upper side begun at the west end. This might be accounted for by supposing that the boulder had been pushed towards the east over hard rocks, and that floating ice from the westward had pushed stones over the upper surface. The smallest end of boulder pointed towards west. The sides of the boulder were well rounded.

This boulder lay in a hill of muddy sand containing many pebbles of all kinds, hard and soft, such as quartz, shale, and coal. Height above sea, 200 feet.—*Ed. Geol. Soc. Tr.*, vol. ii. p. 347.

PEEBLESSHIRE.

At the east end of the town of Peebles there is a boulder of white quartz, about 3 feet long, $2\frac{1}{2}$ feet broad, and with a girth of about 7 feet. It is now built into a wall. Previously to its being thus disposed of, the stone stood from time immemorial in an adjoining low hill, which in consequence had obtained the popular name of the "White Stone Knowe." It is alluded to as a boundary stone in a title deed dated in 1436. Mr Richardson, the Secretary of the Edinburgh Geological Society, who was the first to take public notice of this boulder, says that "the nearest beds of quartz are about 80 miles to the N.W." The boulder on its surface is smoothed and polished. It is, like many other boulders, rudely pointed at one end, whilst the other extremity is more broad and heavy. The height above the sea is 550 feet.—*Ed. Geol. Soc. Trans.*, vol. ii. p. 397.

PERTHSHIRE.

1. *Loch Tay.*—On the farm of Morenish, situated on the north bank of the lake, and about 2 miles from the village of Killin, there are several boulders worthy of notice.

Figs. 13, 14, 15, Plate II., are intended to show the positions and specialties of these boulders. They were at a height of about 1400 feet above the sea, assuming Loch Tay to be 300 feet.

These boulders had all come from the westward, viz., down the valley, as shown by the way in which they were fixed.

If the question be, whether they were brought by glacier or by floating ice, the answer is, that there is not much evidence either way. It may however be remarked, that if they were pushed forward by a heavy glacier, it is odd that the boulders should not have been carried further down the valley, and that the obstructions on their east side, against which they have stuck, should not have yielded under the pressure of a ponderous glacier. The boulders in figs. 13 and 14 were resting on detritus, and pressing against detritus only on their east sides. The boulders in fig. 15 was pressing against a hard rocky stratum of clay slate on its east side.

In several parts of the hill, smoothed rocks of mica schist occur, with knobs of quartz standing up above the general surface. Being harder than the mass of rock, they had resisted the friction better; these knobs were smoothed, the smooth parts being always on the west sides.

Fig. 16, Plate III., shows a rock with joints. The projecting angles facing the west have been smoothed by some abrading and grinding force.

2. *Glen Dochart.*—There are many boulders of considerable size, resting on detritus, and chiefly on the south side of the valley.

One near an old toll-bar measured, in so far as above the ground, $13 \times 12 \times 8$ feet, at a height of 630 feet above the sea.

At the small farm-house of Wester Lix, at a height of 660 feet above the sea, there is a flat or terrace, partly rock, partly detritus, on which there are several large well-rounded boulders, two of them a coarse granite, probably from Ben Cruachan.

On ascending the hill towards the south, a boulder, $12 \times 9 \times 5$ feet, was met with, at a height of 1116 feet above the sea. Its longer axis bore E. $\frac{1}{2}$ S., which is also the direction of the axis of the valley in this place. There being no rocky hill near, from which this boulder could have come, it has certainly been brought to the spot where it now lies, by some transporting agent.

At the height of 1250 feet there is a mass of rock on the same side of the valley, and nearer the top of the ridge, which has on it some noteworthy marks. The rock stands out prominently, and forms a nearly vertical cliff, as shown in fig. 17, Plate III. On the side

facing the west, there are horizontal groovings, apparently formed by some force, which, acting on the whole mass, has worn down certain portions more than others, these portions being less compact, and so more capable of abrasion.

Such abrasion might have been effected by a body of water passing from the westward; and more readily, than by the solid ice of a great glacier.

On the top of the ridge forming the south bank of this valley (GlenDochart), a cairn stands at a height of 1500 feet above the sea. A boulder of considerable size lies on the top of this ridge, on the east side of a projecting knoll. Has the boulder been stranded on what was the lee side of the knoll?

ROSS-SHIRE.

At *Auchnasheen* (Dingwall and Strome Ferry Railway,) there is a boulder about 15 feet in girth, which stands on a flat of detritus about 610 feet above the sea.

In this district, there are several other detrital flats, in sight of this one, all nearly on the same level. There can be no doubt that these flats have been originally one continuous plateau, which formed a sea-bottom. It has been cut through by several streams, the banks of which, about 18 feet high, show an enormous accumulation of gravel and sand;—sand, *below* (deposited probably when the water was deep); gravel, *above* (deposited when the water was shallower and more subject to currents).

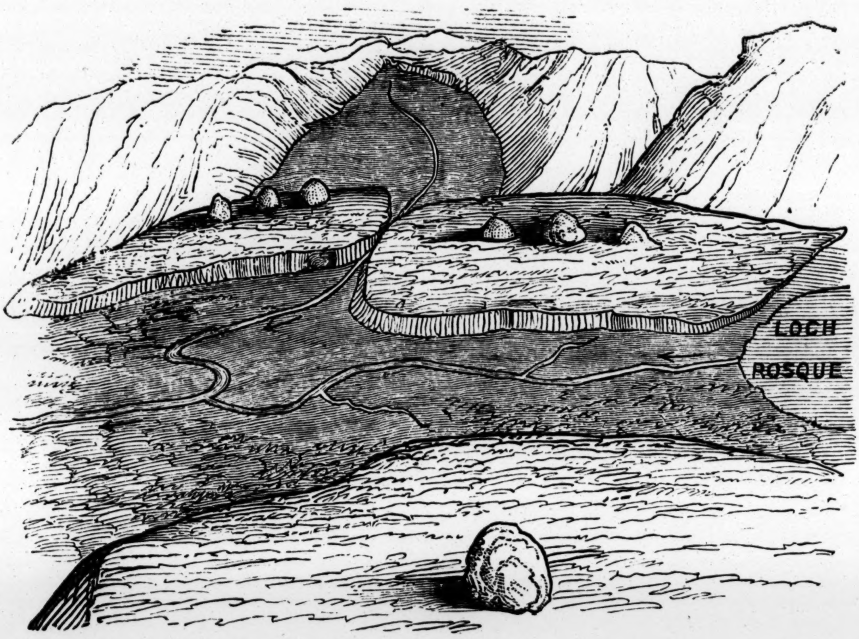
The annexed diagram represents a portion of these remarkable flats, —cut through by several streams, the principal of which flows from Loch Rosque,—situated to the north of the boulder. The knobs on the woodcut are intended to represent knolls of gravel or sand—remnants of a greater mass of these materials. The boulder is well rounded, and it has evidently come from a distant quarter.

Professor Nicol of Aberdeen has expressed an opinion * that the formation of these Auchnasheen terraces is due to the action of a great river flowing from the west. I regret to differ on this point from a geological friend; but I can see no grounds for that opinion.

To the east of Auchnasheen, close to the railway, there are several spots of rock evidently rounded by friction—whether by ice or by

* Nicol's Geology of Scotland, p. 69.

water, or by both, it is difficult to say. Their height above the sea is about 780 feet. On the hills, on each side of the railway, there are traces of horizontal lines on the detritus, which deserve better observation than could be given from the railway carriage.



STIRLINGSHIRE.

1. *On Sheriffmuir*, 3 miles from Bridge of Allan, near Blackford, there is said to be a large boulder, called Wallace's Putting Stone.

NORTHUMBERLAND.

It was intended that only Scotch boulders should be inquired after by the Committee; but it is not irrelevant to notice a boulder which, though now in England, was probably transported from a Scotch mountain.

In Chillingham Park, the seat of the Earl of Tankerville, near Alnwick, there are several small boulders of granite. The rocks of the immediate neighbourhood are carboniferous sandstones and limestones. The nearest point for granite is the "Big Cheviot," eight miles to the W.N.W., and reaching a height of about 1800 feet above the sea. The largest boulder is 3 feet 2 in length, 2 feet

4 in width, and 2 feet high. It is round in shape, and about 400 feet above the sea.

Several valleys and ridges of hills lie between Chillingham and the Big Cheviot, across which the boulder must have been transported to reach its present site.

Remarks by David Milne Home, LL.D., Convener of the Society's Boulder Committee, on presenting the Committee's Fourth Report at a Meeting of the Society, 20th May 1878.

1. In presenting a Fourth Report from the Society's Committee on Boulders, I may be allowed, first, to refer to the main object for which the Committee was appointed.

It was to collect data which might help towards a solution of the problem, by what agency boulders in Scotland had been transported from the parent rocks to the positions they now occupy.

The Transactions of the Society contain numerous papers by eminent geologists on this question.

At a very early period, Sir James Hall, when he drew attention to many large boulders, and also to the remarkable appearances called "crag and tail" in the midland districts of Scotland, ascribed both sets of phenomena to the agency of great bodies of water, which had passed over the country from west to east.

At a later period (about the year 1842), Agassiz and Dr Buckland started the idea, that as in Switzerland, glaciers had been the means of carrying masses of rock from the Alps across the valley of Geneva to the Jura mountains, so there might in former days have been glaciers in Scotland producing similar effects.

More recently a third theory was started,—that if the sea stood several hundred feet above its present level, floating ice might have been the means of transporting the boulders, and carrying them great distances.

2. There being thus three different theories of transport, each supported by eminent geologists, the Committee has endeavoured to gather facts to ascertain which theory is the most probable, or whether any better can be suggested.

I do not presume to say that the information contained in this and

the previous Reports will yet allow the problem to be solved. But at all events it may be conceded that some new facts have been ascertained, which throw considerable light on the question.

I venture to indicate what appear to me to be several conclusions warranted, though in doing so I offer only my own opinion. Perhaps the Committee, after more information has been obtained, may be induced to consider whether they will pronounce on the various questions of interest which the subject presents.

I confine myself this evening to only a few points, and chiefly to illustrate what occurs in our last Report.

3. The boulders referred to in the Report may be divided into two classes.

First, There are boulders which, from the nature of the rock composing them, are so soft and friable, that they could have been transported only short distances—such as sandstone, coal, and shale. In the Report, examples are given of such boulders, from Berwickshire and Mid-Lothian.

The *second* class of boulders, namely, such as are ascertained to have come from remote quarters, are composed of rocks, hard, compact, and homogeneous in composition; such as basalt, greenstone, granite, felspar, quartz, greywacke, and old conglomerate.

Boulders of these rocks have been found even as far as 80 or 100 miles from the parent rocks; and, generally speaking, they are well rounded, presenting evidence of enormous friction undergone whilst *in transitu;* and even in some cases acquiring almost a spherical shape.

Specimens of small spherical boulders are now on the Society's table.

There are, however, exceptions to the rule that boulders of hard compact rocks are generally well rounded. Cases of boulders of these hard rocks occur extremely angular in shape. Examples are shown in this Report, by the lithographs appended to it, and in previous Reports. These *angular* boulders are almost invariably at *high* levels, on the sides of mountains or near their tops. The *well rounded boulders* are generally at *low* levels, and most frequently imbedded in boulder clay.

4. It will be asked, whether the Committee has in any case ascertained the parent rock from which a boulder has come.

The answer is, that the Committee can in no case point out the particular rock from which a boulder had originally been broken off. All they can affirm is, that in several cases they have ascertained the *district* or *quarter* from which the boulder must have come.

(1.) For example, in Berwickshire, as will be seen from this last Report and the second Report, particular hills are specified from which boulders must have come. The direction in which they came, and the number of miles traversed, are therefore in these cases matter of certainty. In every case over the whole county of Berwick, from its lowest to its highest level, the direction of transport is from points between W. and N.W. (magn.)

The same is the case in Mid-Lothian. The sandstone boulders at Craiglockhart are shown to have most probably come from rocks situated a few miles to the N.W. The quartz and other hard rock boulders at the same place, as also at Liberton and at Leith, in like manner probably came from points between W. and N.W.

(2.) The two remarkable spherical balls of marcasite, found in the boulder clay at Leith and mentioned in this Report, must in like manner have come from the westward. A presumption to that effect arises, from the mere fact that they are in the same bed of clay which contains granite and other Highland rocks. But there is more than presumption. Mr Peach having indicated where pyrites balls might be found *in situ*, viz., at Campsie and Kilsyth, I went to Campsie last week, and on inquiry was shown some thin strata of coal, abounding in nodules of pyrites, several of the nodules so large as to weigh half a cwt. The coal is worked for burning limestone. It is too full of sulphur for domestic use. Specimens of this coal, with the pyrites nodules which I obtained on the spot, are now on the table of the Society.

Kilsyth I did not visit, because the overseer at Campsie told me that he had worked at Kilsyth, and that there were pyrites nodules in the coal strata there, similar to those at Campsie, but of rather smaller size.

Some of the nodules which I obtained at Campsie I submitted to Professor Crum Brown, that he might examine them to see whether they contained "marcasite." He has reported to me as follows:—"These nodules have a specific gravity of 4·12, and consist of iron, sulphur, and coaly matter in the following proportions:—

"Iron, . . .	44·56	per cent.
"Sulphur, . . .	52·14	"
"Coaly matter, . .	3·30	"

Deducting the coaly matter, the iron and sulphur would be in the proportions in which they are generally found in 'marcasite,' viz.,

"Iron, 45·61; and Sulphur, 54·29."

As regards chemical compositions, therefore, the small metallic boulder may be considered as exactly agreeing with the nodules found in the Campsie coal strata. This agreement in composition affords a strong ground for inferring that the boulder had been transported from Campsie, or from Kilsyth, as suggested by Mr Peach.

With regard to the larger spherical ball found in the same bed of boulder clay at Leith, I am now able also to indicate the part of the country from which it was probably transported. Mr Hutchison of Carlowrie, happening to see this stone ball, informed me of two quarries in Linlithgowshire where concretions resembling it were in abundance. These quarries are near Humbie and Dalmeny, situated from nine to ten miles due west from Edinburgh. Mr Hutchison having sent to me several of these concretions, I was induced to visit Dalmeny Quarry. I found in the sandstone rock there, numerous concretions of all sizes up to nearly 4 feet in diameter. Humbie Quarry I did not visit, as the working of it had been given up, and it was full of water. A concretion from this last mentioned quarry, sent to me by Mr Hutchison, Professor Crum Brown has examined, with the following result:—"It weighs 17¼ lbs. It consists externally of a thin shell of sandstone, and internally of a mixture of quartz and marcasite, closely resembling the substance of the large ball from Leith. The mean specific gravity of the ball was 3·49."

There is thus a sufficient similarity of composition in regard to the stone ball and the Humbie concretions, to make it exceedingly probable that these Humbie sandstone rocks supplied the stone ball. I do not say that Humbie Quarry was the exact spot from which the stone ball found at Leith actually came. The sandstone strata which occur at Humbie and Dalmeny of course crop out elsewhere in the district near South Queensferry; all that can be said is, that

the stone ball may have come, and most probably came, from some part of that district. Mr Peach mentions in his letters, quoted in the Report of the Committee, that similar concretionary balls occur in sandstone rocks near Burntisland, and suggests that the ball in question came from that quarter. In that case, the direction of transport would be from about due N. If the stone came from near South Queensferry, the direction would be from W.N.W., which last would be more in accordance with the evidence of direction indicated by many other data.

Assuming, then, as most probable, that the large stone ball, as well as the small metallic ball found in the Leith boulder clay, came from parent rocks, situated to the westward, the next question will be, by what agency were they transported?

Mr Peach, in his letter, apparently assumes, as matter of course, that these balls were transported by the agency of *ice*. But "ice" in what form?—land-ice, or sea-ice?

If the metallic boulder came from Campsie, the distance over which it travelled to Leith could not have been less than 30 miles; and as the Campsie coal strata are only about 150 feet above the present sea-level, there would not be gradient sufficient for a *glacier* either to carry on its surface, or to push before it, debris of rocks from Campsie to Leith. Moreover, Leith is not at or near the mouth of any valley which could create or guide a glacier from the west of Scotland.

But there are in the Campsie and Kilsyth districts marks of various kinds, indicating the action of a deep-sea current. These marks it is proper to notice, as having an important bearing on the general question of boulder transport.

Mr John Young of Glasgow, in the year 1868, wrote an instructive paper in the "Transactions of the Glasgow Geological Society," on the geology of Campsie. He says (page 14)—"There are few localities in the central district of Scotland, where such an extent of polished and striated rock surface is to be seen, as along the flat summits of the south hill of Campsie. The striæ vary in their direction from a few points north of west to south of west, according to the deflection of the ground;—many tracts of the sandstone rock, still showing the channelled markings in great perfection," at about 600 feet above the sea.

Mr Young then refers to the Strathblane Valley, which lies between the north and south hills of Campsie, and to the appearances indicating that it had been "*swept by powerful currents of water*, which have helped to produce those inequalities of surface seen along the outer margin of the tracts now occupied by the rivers Kelvin and Glazert. It was during the period when Scotland sat several hundred feet lower in the sea than it does at present, and when *the valley of the Kelvin existed as a deep sound connecting the German and Atlantic Oceans*, that those great beds of stratified sand and gravel were deposited which we now see filling up the Strath (as near the village of Torrance) to more than 100 feet above the level of the river. At other points along its course, similar deposits exist to more than 100 feet *below* the present sea-level. This shows that a *very deep sound or valley must have originally extended across Scotland, previous to the glacial period, in this particular direction.* A depression of the land to the extent of 350 feet would produce the following results:—The German and Atlantic Oceans would be united by the valley of the Kelvin, also by the valley of the Leven, Loch Lomond, and onwards by the low ground near Kippen to the Forth at Stirling. *A narrow sound through the Campsie valley* would connect the two seas, as the water-shed at Ballagan Bridge is only 330 feet. *The Campsie and Kilpatrick hills would then form two islands*, and the valleys of the Carron and the Endrick would be estuaries or arms of the sea. It is only by assuming conditions such as these, that we can hope to explain the superficial sedimentary deposits" (page 16).

In the year 1871, in company with Mr Young, I had an opportunity of visiting the Campsie district, and from my note-book I make the following extracts:—

a. On Craigend moor, at about 450 feet above the sea, situated two miles west of Strathblane, I found the sandstone rock presenting extensive sheets of smoothed horizontal surface, evidently ground down by friction, and presenting occasional striæ, running in a direction S.E. by S. The rock had in some places imbedded in it quartz pebbles, standing up above the general surface. Being harder than the sandstone rock, these pebbles had been able to withstand the friction; but some of them showed marks of rubbing on their north-west sides.

b. At this place, looking towards the N.W.—viz., in the direction of Loch Lomond—an opening between the hills, which are apparently about 1000 feet high, was discernible; this opening being about 1½ mile wide.

c. At four other places on Craigend moor, from 500 to 600 feet above the sea, two to three miles apart, there were striations on the rocks, pointing repectively S.E. by S., S.E. ½ S., S.E. by S., and S.S.E.

At all these places the direction was seen to pass through the opening between the hills above referred to, indicating that the agent, whatever it was, which produced the striations might have come, and probably came, by that opening.

d. On this same moor (forming an extensive plateau of about 6 miles long by about 3 miles wide) I had pointed out to me by Mr Young several boulders in different places.

Two were of trap, from the Kilpatrick hills, situated some miles to the W.N.W., and at a height of 570 feet above the sea. In circumference, each boulder measured 27 feet, and, so far as not buried in the drift on which they were lying, the height of one was 4½ feet, of the other 6 feet.

Another boulder, well rounded, 500 feet above the sea, was of grey granite, weighing about 2 cwt., which Mr Young considered, from the size of its felspar crystals, to have come from Ben Awe, a mountain situated to the N.W., and distant about 50 miles.

There were several smaller boulders of old conglomerate—transported, no doubt, from the well-known band of that rock which, running from Dumbarton, crosses Loch Lomond in a N.E. direction towards Aberfoyle.

e. In the valley of the Blane there are deep beds of sand formed, most probably, whilst the sea occupied the valley, and numerous well-rounded boulders of all descriptions. At Strathblane Railway Station there was a deep cutting of a sandbank, with several boulders in the sand, and one in such a position as to indicate that it had fallen from some raft which had been conveying it, as it was sticking with its narrowest point downmost.*

f. It was remarked to me by Mr Young, that whilst boulders,

* See a diagram of this sandbank and boulder in a little book, published by Edmonston & Douglas in 1871, called "Estuary of the Forth."

gravel, and beds of sand are abundant in the valleys of Strathblane and Campsie, he had never found any marks of grinding or striation on the rocks in these valleys. These effects seemed to have been produced at levels higher than 400 feet above the sea.

On another occasion, when geologising on the Campsie hills, above Glorat, situated 3 miles to the east of Campsie, and at a height of 800 feet above the sea, I found the sandstone rock striated, in a direction due E. and W. On the Kilsyth hills, a few miles still farther east, and at a height of 1200 feet above the sea, the striations on the rocks were seen to be E. and W.

g. One other fact observed was the immense accumulation of boulders of all kinds at Croyhill, a knoll of trap, at the summit level betweeen the firths of Clyde and Forth—viz., about 160 feet above sea-level. As some of these boulders were of "*old conglomerate*," they afford additional evidence of an agency which brought them from the westward.*

h. In addition to these facts, notice may be taken of two boulders reported to the Committee by Mr Jack of the Geological Survey. One is of mica slate, weighing about 6 tons, on the Kilsyth hills, at 1260 feet above the sea, the parent rock of which Mr Jack supposes to be situated about 15 miles to the north. The other is of conglomerate, weighing about 7 tons, on the north hill of Campsie, at 1803 feet above the sea, with its longer axis W. 20° N. Its parent rock is supposed by Mr Jack to be to N.W. (First Report of Committee, p. 51.)

Now what do all these facts prove? They prove that an agent of some kind or other moved over this district, having a depth of at least 1800 feet, and covering a great breadth of country; and that, whilst this agent was moving, the rocks over which it passed were ground down and rutted and striated; large boulders, at a high level, were carried forward, and boulders at a low level were pushed in a similar direction.

There is an additional fact deserving notice. The valley at Lennoxtown, where the pyrites coal strata are worked, seems to have at one time been filled up by these strata. These strata now, however, exist only on each side of the valley. Some agent has scooped them away, whereby the present valley was excavated; and it is

* "Estuary of the Forth," p. 95.

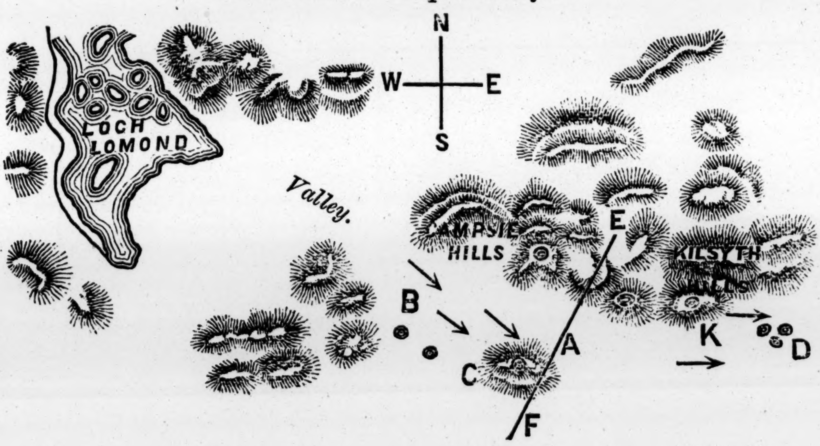
possible that the balls found in the Leith boulder clay form a portion of the debris of these pyrites strata so broken up.

What agent can fit into all these conditions so well, as a sea current loaded with ice?

On this theory, it is intelligible why the rocks along the moors of Craigend and Craigmaddie, stretching for 5 or 6 miles in a direction S.E. and S.S.E., at a level of from 500 to 700 feet above the sea, should show more effects of grinding and striation than the rocks at a lower level. Had a glacier been the agent, the grinding would have been chiefly at the lowest, not at the highest levels.

The subjoined plan and section of Campsie hills and valley will make the foregoing explanations more intelligible. The plan is copied from a published map by Johnston. The section has been

Ground Plan of Campsie Valley.



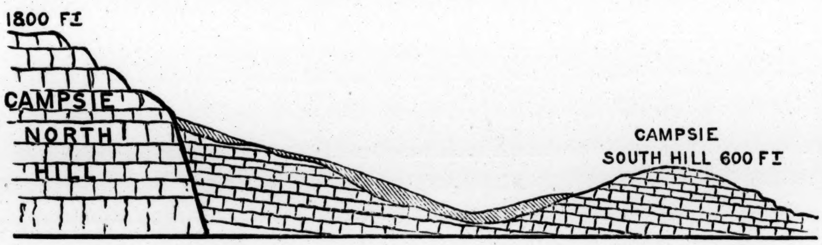
A, Pyrites coal strata, out-crop of.
B, Craigend Moor, 450 feet.
C, Craigmaddie Moor, 700 feet.
D, Boulder and striated rocks at Croyhill.
K, Kilsyth coal strata.

Boulders shown by black dotts.
Striæ on rocks by arrows.
FAE, Line of section across Campsie Valley.

kindly drawn for me by Mr John Young of Glasgow, who is thoroughly well acquainted with the geology of the district. In his letter sending the section, Mr Young says—" The Campsie coal and limestone is at present worked on the flank of the north hill, as well as in the mine which you saw in the south hill. The valley

between these hills is one of denudation. Several hundred feet of strata, belonging to the Lower Carboniferous Limestone series, have been removed, or scooped out by currents of the ocean.

"If you examine Sheet 8 of the horizontal section of the Geological Survey (by Professor Geikie) you will find on the south hill of Campsie the outcrop of the coal and limestone. This sheet shows, quite as distinctly as my sketch, the valley running between the south and north hills, and the great denudation of the coal strata containing the marcasite balls."



Section across Campsie Valley ; coal and limestone strata overlaid by gravel and earth.

These explanations go far to show how the small marcasite ball found in the Leith boulder clay probably came from Campsie. A geological study of that district indicates the agency of deep-sea currents loaded with ice, which flowed upon the Campsie hills from the W.N.W., scooping out the valley which now occurs there, and breaking up to a large extent the coal strata in that valley. The debris of these strata would be swept along to the eastward; and some of the nodules forming part of these strata would be buried in the boulder clay now existing at Leith.

4. The cases which I have just been describing are of boulders, large and small, which have come from remote places, now separated by an intervening tract of *dry land* from the present sites of these boulders.

(1.) But there are cases of boulders which to reach their present sites must have crossed *arms of the sea*, even now of considerable depth and extent. In such cases, the theory of local glaciers is, of course, scarcely conceivable.

Thus on the Island of Islay, the Committee's last Report refers to several large boulders of rock, differing from any rock known in

the island. At least, such was the opinion I formed after a week's ramble, and after inquiring among intelligent persons well acquainted with the rocks of the island.

So also in the Island of Kerrera, opposite to Oban, there are numerous blocks of grey granite, though no rocks of any kind of granite occur in the island.

On the small island of Staffa, consisting entirely of basalt and greenstone, I found boulders of red granite and gneiss, which probably came from the Mull mountains, situated to the N.E. (Committee's 2d Rep., p. 157).

(2.) In Nairnshire there are many conglomerate boulders of huge size, and angular in form, which must have been transported across what is now the Cromarty Firth from Ross-shire. They are at a height of from 400 to 600 feet above the sea. (First Rep. of Committee, p. 42.)

Other examples are afforded by the black granite boulders at Appin and in Loch Creran. Specimens of these are now on the table. As the present Report gives a full explanation regarding these boulders, I do not require to repeat how, when these specimens were submitted to Professor Judd of London, who has made the igneous rocks of the West Highlands a special study, he gave his opinion that there was no rock of the same description on the mainland, and that it was to be found only in Mull. From that island, therefore, these boulders must have been transported, and across a sea, which even now has at one place a depth of 100 fathoms, but which transportation probably took place at a period when the sea stood hundreds or even thousands of feet above its present level, or when the land sat that much lower in the ocean.

(3.) If Professor Judd's opinion of the Loch Creran and Appin boulders be correct, it goes far beyond an explanation of the boulders in these localities. For example, the Island of Lismore, whose rocks are entirely limestone, has on it many boulders of granite, which probably also came from Mull, inasmuch as Lismore lies between Mull and Appin (Com. 2d Rep., p. 157). In Lochaber there is the hill called Craig Dhu, about 2000 feet in height, so called, I believe, from the great number of black granite boulders resting on and near its top. These boulders, on account of their peculiar colour as well

as position, attracted the notice of Professor Nicol and Mr Jamieson; and they are mentioned in both of my recent papers "On the Parallel Roads." (See also Committee's First Report, p. 39.)

In these papers I had occasion to point out how the position of the boulders both in Glen Roy and Glen Spean indicated that they had come—not *down* these glens, but *up* the glens. If the boulders at Loch Creran were rafted on ice from Mull by a sea current flowing eastward, the position of the boulders in Glen Roy and Glen Spean could be explained in the same way.

(4.) There is another fact connected with the position of boulders in the West Highlands, and indeed over Scotland generally, which receives explanation from Professor Judd's paper "On the Ancient Volcanoes of the Hebrides," I mean the high position of many large boulders.

In the Committee's Second Report notice is taken of a remark by the Ordnance Surveyors (p. 157), that in the Stratherrick district, where the highest hills are about 2900 feet above the sea, the boulders on the sides of these hills extend down to a level of about 2250 feet, *but not lower*.

In Fortingall parish (Perthshire) a gneiss boulder, weighing above 400 tons, is lying on clay slate rocks at a height of 2500 feet, being very near the ridge of clay slate hills. The gneiss hills form a range about 20 miles to the north and north-west. (Committee's First Report, p. 49.)

On the Fannoch Mountains (Ross-shire) a gneiss boulder of about 130 tons weight lies on a water-shed at a height of 2000 feet above the sea. (Committee's First Report, p. 49.)

On Schehallion (Perthshire) blocks of grey granite are seen at a height of 3000 feet. (Committee's Second Report, p. 173.)

On the top of a hill in Lochaber, exceeding 3000 feet above the sea, there are granite boulders. (Paper on Parallel Roads, Tr. of Soc. vol. xxvii. p. 740.)

Now where are there at present in Scotland ranges of mountains from which fragments could have been transported to such heights as those above named? There are now none such. Isolated peaks there are, but none exceeding 4300 feet; and of these there is but one, in the West Highlands (Ben Nevis), though it is from the westward that the great bulk of the boulders which overspread Scotland have

come. Professor Judd's paper, giving reasons for believing that there were in Pliocene times mountains in Skye, Mull, Ardnamurchan, and even in Rum, some of which reached to a height of at least 14,000 feet, solves the difficulty, and explains many other curious facts besides.

For example, there is a series of granite boulders containing unusually large crystals of quartz, felspar, and mica, which occupy the straths between Fort-William and Kingussie. A boulder near Fort-William is 1500 feet above the sea, and from its position appears to have necessarily alighted on the hill from the westward (Committee's 2d Report, p. 161–2). If the sea stood at 2000 feet or more above the present level, the valleys of Lochaber and the Spey would be occupied by sea, and through them a current could flow from the ocean on the west to the ocean on the east. The summit level now between Lochaber and Strathspey is 850 feet above the sea, so that if the climate at that time was such as to allow of glaciers among the mountains and of floating ice on the sea, there would be means of transporting boulders from Mull to Lochaber and Strathspey.

5. There are several other instructive features connected with boulders brought out in this as well as in previous Reports.

(1.) The different shapes of boulders.

The Appin boulders are round shaped, whilst the Loch Creran boulders are angular, though the rock composing them is the same. The former are known in the district as "the round stones of Appin."

These Appin boulders are on the shore of the Linnhe Loch, through which in former times there must always have been a rapid current flowing, between the high mountains, forming the Glen-na-Albin or Great Glen of Scotland.

If icebergs then floated on the sea, these boulders must have undergone much pushing and rolling; whereas the Loch Creran boulders, being in what would then be only an arm or inlet from the main channel, would be exposed to no such friction.

In reference to the Kyle or sea strait, in what is now the line of the Caledonian Canal, the grinding to which the rocks on the sides of the valley have been subjected, is well seen at Cullochy on the north side, and at Inverfarrignig on the south side of the canal.

(2.) Another common feature presented by boulders in Scotland is, that when they are longer than they are broad, the longer axis is parallel with the direction in which the boulder had been transported. Very frequently also, when one end is sharp and the other end broad, the former points towards the direction from which the boulder has come. On the theory of icebergs and floating ice this feature is intelligible; on any glacier or ice sheet theory it is not.

(3.) The existence of striæ on boulders, and the circumstance that these striæ are sometimes deeper at one edge than on the rest of the surface, is a new fact brought out in this last Report (page 688).

6. In several parts of the Report allusion is made to the evidence which boulders seem to afford, of the enormous denudation which there must have been in the district where these boulders are situated (pp. 662–667).

7. Notice is also taken in two districts of the West Highlands of horizontal terraces on the sides of hills, up to a height of 1800 feet above the sea.

If these are to be ascribed to sea action, as suggested in the Report, they would only show that Scotland possesses the same features in this respect as Norway, Sweden, and America, where there are horizontal terraces to even greater heights. It is only reasonable to expect that in the north of Scotland such records of the ocean should be discernible, considering the enormous beds of sand and gravel found at great heights in many of the mountains. On Schehallion (Com. 2d Rep., p. 173) there is gravel up to a height of at least 3000 feet.

In reference to the suggestion, that these terraces on the sides of mountains in the Highlands are marine, it is not unimportant to observe, that similar horizontal terraces at high levels occur also in lowland districts. Mr James Geikie, in his "Great Ice Age," refers to a series of "high level terraces of gravel and sand at Eaglesham," about 12 miles S.W. of Glasgow, the highest being 800 feet above the sea. "I have also traced them," he adds (page 248), "on the Moorfoots, up to 1050 or 1100 feet; and these, like the Eaglesham beds, seem equally to require the agency of the sea. Still farther south, high level shelves of gravel and sand have been

detected by my colleague, Mr Skae, in Nithsdale, at a height of 1250 feet above the sea."

8. Lastly, may I be permitted, as there is still a wide field for farther investigation, to express a hope that the Boulder Committee may be re-appointed, and with additional labourers to carry on the work. I will be happy to be allowed to remain on the Committee, but I wish to resign the honour of being Convener. I begin to find that I am now not able for the hill-climbing and trudging across Highland moors and morasses, which boulder-hunting requires.

Edinburgh, 24th May 1878.

At a Meeting held this day, the Council re-appointed the Boulder Committee, with the addition of Dr Andrew Fleming, M.D.; William Jolly, Inspector of Schools, Inverness; and Ralph Richardson, Secretary of the Edinburgh Geological Society; and agreed to express a hope, that Mr Milne Home would continue Convener of the Committee.

The Council further agreed, that the "Remarks" by Mr Milne Home, at the Society's meeting on the 20th inst., when he presented the Committee's Fourth Report, appear in the Society's Proceedings, along with the Report.

J. H. Balfour, *Secretary*.

The Boulder Committee now consists of the following Fellows:—

Sir Robert Christison, Bart.
Sir Charles Wyville Thomson.
Rev. Thomas Brown, Edinburgh.
Dr Andrew Fleming, M.D., Edinburgh.
Professor Archibald Geikie, Edinburgh.
William Jolly, Inverness.
Dr Arthur Mitchell, M.D., Edinburgh.
Professor Nicol, Aberdeen.
Ralph Richardson, Edinburgh.
Thomas Stevenson, C.E., Edinburgh.
David Milne Home, LL.D. (*Convener*).

Monday, 3d June 1878.

Sir C. WYVILLE THOMSON, Vice-President, in the Chair.

The following Communications were read:—

1. On the Splitting up of Electric Currents, as detected by the Telephone, and the founding thereon of a Sounder to call attention from one Telephone to another. By R. H. Bow, C.E.

The telephone is of no use as a "far-speaker," without some means of calling the attention of the attendant at the distant station. Nothing could well be better than the "electric-bell call," and the sounder which I am about to describe makes no pretensions of competing with the bell, except on the points of simplicity, cheapness, and facility of use; and although its employment is limited to short length lines, it may be assumed that it is upon short length lines that the telephone will be most frequently used. I have had this sounder in experimental use for more than three months, and have shown it to many persons as a very obvious expedient. However, as it does not appear to have been referred to in any publication, I venture to bring it as a Note before the Society; it is of too trifling a nature to be made the subject of a formal paper.

In any of the sounders I have seen described, the battery, or other source of electrical excitement, has been placed in simple circuit with the pair of telephones, or put into circuit with the distant one. In the proposed method of sounding, by means of a galvanic battery, the battery is kept separate, and, when used, the short wire from the one electrode is rested against one of the wires of the telephone, while the wire from the other electrode is slid with a very gentle vibrating touch upon the other wire leading from the telephone. On the well-known principle of *derived currents*, we know that the greater portion of the electricity will pass through the shorter or less resisting circuit of the nearer telephone, and yet that there will be a not inconsiderable portion diverted to travel round by the distant one; and this, if the distance be not very great, and

the telephones be of suitable construction, will suffice to elicit sufficient noise at the distant station to call the attention of any one in the same or even an adjoining apartment; the noise given out by the distant telephone will not be altogether due to this diverted current, but will owe part of its volume of sound to telephonic sympathy with the great agitation produced in the one near the operator.

Before entering into any details as to the construction of telephones best suited for this mode of sounding a "call," and for short distances, permit me to describe two experiments upon the detection of derived currents by means of the telephone.

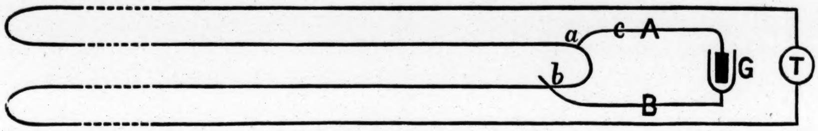


Fig. 1.

Fig. 1 represents a wire of about 320 feet in length, bent so as to bring the middle of its length near to the telephone T, to which its extremities are attached; G is a battery of one cell, and A and B the wires from its electrodes; one of these, A, is fixed to the long wire at *a*; the other, B, is movable at *b*, the point *b* being taken at different distances from *a*. When the length of *a–b* is taken equal to from 3 to 4 feet, the noise emitted by the telephone might be sufficient to act as a call, although only about 1 per cent. of the current from the battery can then pass through the telephone; when the length of *a–b* is reduced to 1 foot, the sound from the telephone may yet be heard several feet away, and as the distance of *a–b* is decreased, the telephone must be brought nearer and nearer to the ear in order to hear the crepitating sound; with *a–b* equal to 3 inches, it may be heard 3 inches off; and when the telephone is held against the ear the distance of *a–b* may usually be reduced to less than a quarter of an inch before the sound is lost. Now, when the length of *a–b* is one quarter of an inch, the strength of the diverted current passing through the telephone used would only amount to the one twenty-thousandth ($\frac{1}{20000}$) of that circulating through the battery. But I have now to note a very puzzling circumstance. Sometimes, under conditions I have not been able

to determine, the crepitating sound was found not to cease even when *b* and *a* were brought together, but continued to be heard when the one wire, B, was moved upon the other A, at *c*. Here there can be no dynamic current produced in the long wire. I shall not detain you with my speculations on the subject; but I should mention that the wire was very imperfectly insulated.

The second experiment gives an interesting practical example of the interference of one pair of telephones with another pair, due to the splitting up of currents. The rough diagram, fig. 2, will serve to explain the arrangement. A and A′ constitute one pair of tele-

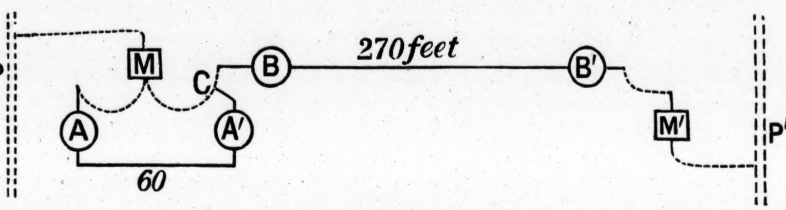


Fig. 2.

phones, the single connecting wire measuring 60 feet; B and B′ are another pair of telephones—these are connected by a copper wire 270 feet long; for the return currents the gas pipes and earth are employed. The gas pipes are indicated by dotted lines; M and M′ are gas metres belonging to different houses; and P and P′ are the gas mains in different streets, and at a direct distance apart of about 400 feet. It will be observed that the gas pipe from C to M is common to the two circuits. Now, it was soon noticed that when the battery-sounder was made use of at A, it caused a sounding not only in A′, as intended, but also in B, sufficiently loud to be heard several yards off if attentively listened for. This noise was, of course, due to a derived current splitting off at C, and making the long circuit round by B, B′, M′, possibly to the gas main P′, and thence to P and back by M, where it rejoins the principal current on its way to A. And using the battery at any one of the telephones causes more or less noise in all the four. But the small amount of sound produced by such straying currents is not likely to cause confusion.

The Telephones used with the Battery-Sounder.—If it were attempted to use the sounder in combination with telephones constructed in accordance with Professor Bell's instructions—that is,

having coils so long as 180 feet of wire so thin as No. 36—comparative failure would result from the very great resistance offered to the current. We must try to do with coils offering an insignificant amount of resistance.

I find that the mode of arranging the parts of the telephone has a great influence on the efficiency, and the construction I have found up to the present time to be the best is as follows:—Taking a 3-inch horse-shoe magnet, I break off about half an inch from one leg; the nick made to indicate the north end is usually at about this distance from the extremity, and the breaking of it there is an easy operation. I then round the unbroken end on a grindstone, and next gum a strip of thin paper round that leg, and upon the paper coil the desired length of silk-covered wire. Taking next a piece of wood 4 × 4 inches or 4 × 5 inches, and half an inch thick, a hole 2 inches in diameter is cut cleanly through it, and crossing the hole is fixed, in letter T fashion, another piece of wood 4 × 3 inches; on this latter piece the magnet is laid with its south or coil-covered end projecting centrally into the hole, and it is fixed down by one screw passing through a small cross piece of wood placed above it. This admits of after-adjustments being very readily made. The ferrotype plate, $2\frac{1}{4}$ inches in diameter, is laid as a cover over the outer aspect of the 2-inch hole, and secured there by the usual ring of wood which constitutes the ear or mouth piece.

With such an instrument placed at A, fig. 2, and another telephone at A′, I have experimented on the length of coil needed. Four or five feet of wire gave very satisfactory results, speaking being heard distinctly, and with sufficient loudness. I then tried shorter and shorter lengths, and found the volume of sound to become reduced certainly, but in a surprisingly small degree; and at last I carried the shortening so far, that only $4\frac{1}{2}$ inches remained as a coil, and though the sound with this fragment was much reduced, giving the voice a far-away character, conversation could still be carried on. I did not think it necessary to push the reduction further, having become convinced that no very great length of wire was needed for the coil to secure distinct hearing, at least for short lines, say of quarter of a mile or less. And for the present I have adopted a length of 9 feet of No. 28 wire (weighing 6 grains per foot length) as a satisfactory coil, presenting a resistance of only

one-third of an ohm, in place of the twenty or thirty ohms when the coil is made up of 180 feet of the much finer wire.

I have had no opportunity of satisfactorily testing the limits to which the sounder is restricted, but it would probably work well enough when the resistance did not exceed ten or fifteen ohms per Daniell cell. A marked advantage is usually found to attend the sending of the currrent in that direction, which increased the power of the magnets of the telephones. And in using the sounder a little skill will elicit a considerably augmented effect.

The principle of split currents may be applied to other purposes; it might offer a ready method of communicating with two adjoining stations on a railway from any intermediate point—that is to say, at any point between the two extreme telephones we could attach a battery or a telephone for sounding to, or communicating with, the attendants at the extreme telephones; or, if a battery be permanently included anywhere in the circuit, a wire alone, at any point on the line, offers the means of sounding the telephones at both stations, the wire being so used as to form an intermittent connection between the earth and the telephone line; and this might perhaps be practicable even from a train in motion, the engine and rails taking part in the earth connection.

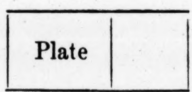
2. An Account of some Experiments on the Telephone and Microphone. By James Blyth, M.A.

Dr M'Kendrick stated, that by applying the microphone or carbon-interrupter of Hughes to the membrane of a phonograph, he had succeeded in using the latter as a transmitting instrument. With such an arrangement, speech could be heard in the distant telephone even after it had become inaudible near the phonograph. He also mentioned that a tambour of Marey, used in physiological experiments, spoke distinctly when the fine point at the end of the lever was applied to the marks on the tinfoil of the phonograph. When a tube was carried from the tambour to the ear, distinct speech could be obtained from phonographic tracings on copper foil, which were scarcely perceptible to the eye. This method also got rid of the difficulty of having the tinfoil impressions quickly rubbed out, as happened when the stillette of the phonographic membrane

was employed. He also introduced to the Society a phonograph made by Messrs Macgillivray and Scobie of Glasgow, which, for loudness, was superior to any one he had yet heard.

3. Note on a Variation of the Microphone. By R. M. Morrison, D.Sc.

Following out a suggestion made by Mr Seabrook in *Nature* of May 30th, I mounted the three carbon blocks of Professor Hughes' microphone on, not as Mr Seabrook recommends, a plate of 3 inches diameter, but on a ferrotype plate about 6 inches by 4. This plate formed part of the top of a box, thus—



This form I found to be extremely sensitive, as a piece of cotton wool ½ inch in diameter falling through 1 inch made a loud sound in the telephone. When the plate was lightly brushed by a camel's hair brush the sound produced in the telephone could be heard a yard away. A small clock placed on any part of the table on which the microphone stood could be heard distinctly ; also a tap on the table, or even walking on the floor, each step producing a clang in the telephone. On speaking into the open part of the box the words spoken were distinctly and loudly heard in the telephone, notwithstanding the difficulty of hearing words spoken by one's self at the same time.

4. On the Action of Heat on some Salts of Trimethyl-Sulphine. Part II. By Prof. Crum Brown and J. Adrian Blaikie, B.Sc.

(*Abstract.*)

In the former paper on this subject, the authors stated that when hyposulphite (thiosulphate) of trimethyl-sulphine is heated to about 135° C., it loses sulphide of methyl to the extent of 23·58 per cent., the salt at the same time fusing to a clear colourless liquid. On cooling, this solidifies to a hard, very hygroscopic crystalline mass.

Analysis agrees with the formula

$$\left.\begin{matrix}(CH_3)_3S\\ CH_3\end{matrix}\right\}S_2O_3.$$

The solution of the substance does not decolourise iodine solution. These results point to

$$\left.\begin{matrix}(CH_3)_3S\text{—}O\\ CH_3\text{—}S\end{matrix}\right\}SO_2,$$

as the probable rational formula of the substance.

Sulphite of Trimethyl-Sulphine.—This salt was obtained by the action of sulphurous acid on the hydrate. It crystallises well, but there is some difficulty in preparing a perfectly normal salt. The salt, as nearly normal as possible, does not, like the hyposulphite, give up its water of crystallisation in the cold over anhydrous phosphoric acid; at 140° C., however, it becomes anhydrous. Heated to 175° C. it gives off sulphide of methyl—8·3 grammes lost 2·32 grammes, or 27·95 per cent. On cooling, the clear liquid residue solidifies, forming a hard, very hygroscopic crystalline mass. This substance was so deliquescent that no analysis of it was made. The mode of formation leads to

$$\left.\begin{matrix}(CH_3)_3S\\ CH_3\end{matrix}\right\}SO_3$$

as its most probable formula.

Note received on July 24, 1872.—In order to ascertain the nature of the crystalline substance obtained by the action of heat on the sulphite of trimethyl-sulphine, the authors converted it, by double decomposition with iodide of potassium, into the corresponding potash salt, which was purified from the iodide of trimethyl-sulphine by crystallisation. This potash salt was found to agree in properties and composition with the "sulpho-metholate," or "methyl-sulphonate" of potash—

$$\begin{array}{c} O \\ \| \\ K\text{—}O\text{—}S\text{—}CH_3 \\ \| \\ O \end{array}$$

The bearing of this fact on the constitution of the sulphites is obvious.

5. On a Class of Determinants. By Mr J. D. H. Dickson, Tutor of St Peter's College, Cambridge.

6. On the Wave-Forms of Articulate Sounds. By Professor Fleeming Jenkin, F.R.S., and J. A. Ewing, B.Sc.

(*Abstract.*)

By the help of the phonograph we have continued the investigation described in a previous Communication (Proc. R.S.E., p. 582), and have now obtained about two hundred magnified traces of the phonographic records of vowel sounds spoken and sung by various voices, and of these sixty-five have been already subjected to harmonic analysis, extending as far as the sixth partial tone. In each case the results have been accepted as satisfactory only when, after the magnified trace had been obtained, the record on the tinfoil of the phonograph still gave the vowel sound satisfactorily. Our attention has hitherto been almost exclusively directed to the vowels *u* (the vowel sound in "food,") and *o* (as in "oh,") both of which are well spoken by the phonograph. The results, which are still incomplete, are briefly as follow:—

When a vowel sound is continuously sung without change of pitch or quality, the wave-form produced is of remarkable constancy, showing that the compound sound does not contain any, or at least any important, constituent which is inharmonic to the prime tone.

A naturally high man's voice, with the comparatively small range *f* to *f'* saying *u* at any pitch throughout its range, produced a wave-form which was substantially a simple harmonic curve of length corresponding to the pitch. The upper partials, when present at all, were present only very feebly. Thus *u* sung on *b*♭ gave a prime whose amplitude was 25 (in the unit of measurement used), the second partial was only 0·8, the third 1·1, and the others inappreciably small.

When the same voice spoke *o* anywhere throughout the same range, it produced a trace which in every case consisted of a strong prime and a strong second partial (that is to say, the octave of the prime), the higher partials being feeble or absent. Experiments on this part of the scale with various voices proved that the proportion which the amplitude of the prime bore to the amplitude of the

second partial might vary greatly at any one pitch, although the sounds were all sung or spoken as *o*, and received by the ear as (generically) that vowel. For example, on *b* one voice gave the ratio of prime to second as 1 to 0·87, while another voice on the same note gave the ratio 1 to 1·8. In any one voice there is not very much change in the ratio in passing from note to note. When the pitch is as high as *d′* or higher, the ratio of prime to second is decidedly greater than on the lower notes of this range. It is probable that the ratio is a minimum for any one voice about the pitch *b♭*, but this is a point requiring further investigation. We have not yet got any satisfactory *o*'s above *f′*.

When, however, the investigation was carried lower in the scale by help of voices of a wider range, several much less simple phenomena presented themselves. Voices capable of singing bass, when singing *u* down the scale gave the usual simple harmonic from above *a*; but, at or near that note a remarkable change suddenly took place in the wave-form given by the vowel sound *u*. At that point it became a duplex wave, with a very small prime, which corresponded to the pitch, and an immensely strong second partial, the ratio of amplitudes being somewhere about 1 to 4. This form continued as the voice went down the scale; but in addition to the very strong second partial a weak third appeared, which became pretty strong on *c*. We cannot say that we have got true and articulate *u*'s at any lower pitch.

The voice of small range mentioned at the beginning of this paper continued to give the single simple harmonic form for *u* down to *f*, below which it could not go. Two other voices experimented with agreed in making the change at or near *a*.

The excessive weakness of the prime in the lower, or what we call the duplex, form of *u* shows how weak a prime tone may be as compared with its upper partials, and yet fix the musical pitch. It also shows how small even the prime may be when not reinforced by oral resonance. The primes of the duplex *u*'s, even when loudly uttered, were absolutely as well as relatively much weaker than those of the *o*'s already described.

The experiments with *u* seem to point to the conclusion that so long as the simple form is given the mouth cavity is adjusted so as to reinforce the prime exclusively, whatever be the pitch. When

the duplex form is reached the cavity must be suddenly changed so as to be approximately if not exactly in unison with the second partial. A voice singing *u* up the scale may sometimes carry the duplex form as high as *b*♭, but the vowel quality of an *u* so spoken is apt to approach *o*. When attempts were made to *slur* either up or down, past the place at which the change occurred, one or other of two things took place—either the sound died away almost completely while the critical point was being passed, or the vowel quality changed form *u* to *o* for an instant. This has been observed both directly and by examination of the magnified phonographic record, in which either an almost blank space or the well known wave-form of *o* might be seen.

An examination of the sound *o* through the lower regions of the scale, and by the help of several bass voices, did not show any such sudden change as took place in *u*, but the number of partials conspicuously present became more and more numerous as the pitch fell. The third partial began to appear about *f*, and on *d* and *e* the prime second and third were all strongly present, the prime being the weakest of the three, and the second the strongest. On *c* the fourth partial was moderately strong, the third stronger, the second stronger still, and the prime weaker than any. The first four partials were all conspicuous on B. On B♭ the fourth was much stronger than any of the others, and the prime was the weakest of the lower ones. The same description applies to A. On G the fourth was still much the strongest, and the fifth appeared weakly. The lower ones were weak. On F the fifth was much the strongest, but the prime, second, third, and fourth were all distinctly present. We have not got any trace good enough for analysis below F. We reserve the numerical result for more detailed publication.

The bearing of our experiments on the theory of vowel sounds is very important. For a considerable range it will be seen that the constituents of *o* and *u* might be simply described, with no reference whatever to absolute pitch—the *u* always being a simple tone, and the *o* a compound of two simple tones an octave apart.

When, however, certain limits are passed, phenomena appear which are evidently connected in some way with an absolute pitch. It seems, however, that this connection is rather due to the neces-

sities of the mouth than to the requirements of the ear; for, as stated above, one voice will give a simple harmonic curve for *u*, while another gives (at the same pitch) a double curve for a sound which is intended for the same letter in the same language, and which is at least generically the same vowel. This fact suggests that the mouth, being unable to shape itself so as to continue the simple form of the letter, adapts itself in some way so as to produce what may perhaps be termed an imitation. It is by no means impossible that this imitation may in some cases be produced by a recurrence to the form used for the same letter at a higher pitch. If this be so, then the hypothesis of a constant cavity for a given vowel sound would be true for the letter *u* when pronounced on notes an octave or a twelfth apart, although not for intermediate notes.

When we examine the sound *o* we find less necessity for insisting on recurrence or any tuning of the mouth cavity. A fair approach to the phenomena observed might be obtained by assuming a constant mouth cavity, having a pitch of maximum resonance near $b'\flat$, as stated by Helmholtz, and reinforcing tones over a large range of nearly two octaves—from f'' to f, or thereabouts. There are, however, some peculiarities in the constituents which suggest that the *o* cavity may also be adjustable. Our results are not inconsistent with the assumption that the cavity is slightly altered or tuned so as to bring the maximum resonance approximately into unison with that upper partial of the prime sung which lies nearest to $b'\flat$. This hypothesis would allow us to diminish the very large range of reinforcement which the constant cavity theory requires, although even an adjustable cavity must still reinforce tones over a considerable range. Further experiments are required to determine how the mouth actually produces the results obtained, but is clear that the idea of relation between the constituents must be combined with that of absolute pitch in any complete vowel theory. The relation may, however, depend for some vowels, though hardly for *u*, on the simple range over which the reinforcement acts. The pitch of maximum resonance in the *o* cavity given by Helmholtz is $b'\flat$, and this, on either hypothesis, agrees well with our results. It is either the pitch of maximum resonance of the constant cavity, or the pitch near which the upper and strongest resonance is to be found, where the cavity is tuned. It may be

observed that, in the case of this letter, possibly the idea of a tuned cavity may be true for singing, and that of a constant cavity for speech.

7. On the Electric Conductivity of the Bars employed in his Measurements of Thermal Conductivity. By Prof. Tait.

The following Gentleman was duly elected a Fellow of the Society:—

Dr J. J. Kirk Duncanson, 8 Torphichen Street.

Monday, 17th June 1878.

Sir WYVILLE THOMSON, Vice-President, in the Chair.

The following Communications were read:—

1. On the Biliary Secretion, with Reference to the Action of Cholagogues. Part II. By Professor Rutherford, F.R.S.S. L. and E., and M. Vignal.

(*Abstract.*)

The method of experiment by which the following results have been obtained has been described in the abstract of Part I., published in the "Proceedings" February 1877. All the experiments were performed on dogs. In the previous abstract the effects of 29 different substances were briefly stated:—

30. Dilute nitro-hydrochloric acid is a hepatic stimulant of considerable power.

31. Jaborandi is a very feeble hepatic stimulant.

32. Calabar bean stimulates the liver, but not powerfully, unless it be given in very large doses.

33. Atropia sulphate antagonises the effect of Calabar bean on the liver, and thus reduces the hypersecretion of bile produced by that substance. It does not, however, arrest the biliary secretion; and when given alone, does not notably affect it.

34. "Menispermin," a resinous matter obtained from the Yellow

Parilla, does not stimulate the liver. It slightly stimulates the intestinal glands.

35. "Baptisin," a resinous matter derived from the *Baptisia tinctoria*, is a hepatic and also an intestinal stimulant of considerable power.

36. "Phytolaccin," a resinous matter prepared from the *Phytolacca decandra*, is a hepatic stimulant of considerable power. It also slightly stimulates the intestinal glands.

37. Sodium Benzoate is a powerful stimulant of the liver. It does not stimulate the intestinal glands.

38. Ammonium Benzoate stimulates the liver, but not quite so powerfully as the sodium salt of benzoic acid. It does not stimulate the intestinal glands.

39. Benzoic acid stimulates the liver, but owing to its insolubility, its action is less rapid and much less powerful than that of its salts.

40. Sodium Salicylate is a powerful hepatic stimulant. It does not notably stimulate the intestinal glands.

41. Ammonium Phosphate is a moderately powerful stimulant of the liver. It is not an intestinal stimulant.

42. Tannic acid does not affect the biliary secretion.

43. Hyosciamus does not notably affect the biliary secretion, and does not prevent such a stimulant as sodium salicylate from augmenting it.

44. Morphia does not affect the biliary secretion, and does not prevent the stimulating effect of such a substance as sodium salicylate.

45. Pure diluted alcohol does not affect the biliary secretion.

46. Potassium iodide does not affect the biliary secretion.

47. Veratrum viride has no notable effect on the biliary secretion.

48. Manganesium sulphate is not a hepatic stimulant. It powerfully stimulates the intestinal glands, and like other purely purgative agents, such as magnesium sulphate and gamboge, it indirectly lowers the biliary secretion.

49. Ailanthus glandulosus is an intestinal but not an hepatic stimulant.

50. Acetate of lead somewhat diminishes the biliary secretion, probably by a direct action on the liver.

51. "Hydrastin," a resinous matter, prepared from the root of the *Hydrastis canadensis*, is a hepatic stimulant of considerable power. It also slightly stimulates the intestinal glands.

52. "Juglandin," a resinous matter, prepared from the root of the *Juglans cinerea*, is a hepatic stimulant of considerable power. It also slightly stimulates the intestinal glands.

Thus, by means of a new and precise experimental method, the physiological pharmacology of one of the most important organs of the body has been in this research worked out as far as at present it appears desirable to proceed, and knowledge that is definite and reliable, because obtained by a method of accurate measurement, after the elimination of disturbing factors, has by this research been substituted for the vague traditions of the past.

The effects of fifty-two medicinal agents upon the liver have been investigated, and the vast majority of the conclusions are in complete harmony with the results of clinical observation. Very many new facts are, however, given to the physician, and even as regards well-known substances, our knowledge of their effects on the liver is now of a character very different from that which previously obtained.

All the experiments relate to the *bile-secreting* and not to the *bile-expelling* mechanism. The authors do not intend to investigate the effects of medicinal agents on the latter, as this point appears to them one of very minor importance compared with the subject of the above research.

The following remarks indicate the position in pharmacology of the above results. Of necessity, the influence of a drug upon a diseased state is the ultimatum of pharmacology, and every experiment upon a healthy bodily system, whether of man or animal, is merely ancillary to experiments with the drug in disease. Having discovered that this or that drug stimulates the healthy liver of a dog, we do not infer that it *must* also stimulate the human liver in health, and still less do we conclude that it must also have this action in disease. The experiments on the healthy liver of the dog, on the normal and on the abnormal human liver, are three sets of experiments closely related, but still distinct. The facts derived from any one of the three sets cannot be substituted for those of the other two. Each set of facts has its own proper place. The above research, therefore,

leaves it to the clinical observer to experiment on man with such substances as sodium benzoate, sodium salicylate, baptisin, euonymin, sanguinarin, &c., and thereby to ascertain whether or not these substances also stimulate the human liver; and of necessity it is also left to him to ascertain in what diseased state the employment of this or of that substance is most advantageous.

Other general conclusions have been already stated at the close of Part I.

2. On a New General Method of Preparing the Primary Monamines. By R. Milner Morrison, D.Sc.

3. On the Preparation and Properties of Pure Graphitoid and Adamantine Boron. By R. M. Morrison, D.Sc., and R. Sydney Marsden, B.Sc.

4. On Colour in Practical Astronomy, spectroscopically examined. By Professor Piazzi Smyth.

Monday, 1st July 1878.

Sir WYVILLE THOMSON, Vice-President, in the Chair.

The following Communications were read:—

1. On the Disruptive Discharge of Electricity. By Alexander Macfarlane, D.Sc., and P. M. Playfair, M.A.

(*Abstract.*)

During the months of May and June of this session, we have endeavoured to investigate certain questions suggested by our experience of the discharge of electricity through the gases and through oil of turpentine.

Ordinary paraffin-oil, when used as a dielectric, exhibits the same phenomena as oil of turpentine. Gas is liberated by the passage of the spark, and at the same time carbon is deposited. Once produced, the gas bubbles make the passage of the spark more easy through bringing the electrified surfaces nearer to one another; hence,

in taking a series of observations, it is necessary to get rid of the bubbles after the passage of each spark. They were attracted generally to the positive surface, but sometimes to the negative. The attraction was more marked when no jars were attached to the Holtz; it was not so powerful as in oil of turpentine, and was generally in the opposite direction.

The electrostatic force required to pass a spark through a layer of paraffin oil or of turpentine is constant, whereas it is variable in the case of air and other gases. For the observed differences of potential plotted with respect to the thickness of layer give a straight line through the origin, while in the case of the gases the curve is concave.

The electric strength of the paraffin oil used was found to be 4, of the turpentine 3·7; air being unity.

To investigate the effect upon the electric spark of heating the electrodes, we constructed electrodes of thick platinum wires placed at right angles to one another—a suggestion we owe to Professor Clerk Maxwell. When one of the wires was heated by a current from a battery of four Bunsen elements, the electrometer deflection was diminished by about one-fourth of its amount, and that whether the wire heated was positive or negative. A similar diminution was observed when the deflection for continued sparks was taken. This diminution of the difference of potential must be due to change at the surface of the wire; for the air between the wires (the shortest distance between the wires being 4 millimetres) cannot be so much rarefied by the heating of the wire as to produce the effect.

We have also investigated the effect upon the electric spark of heating the air round the discs, the pressure being kept constant. We have observed the deflections of the electrometer for a constant spark for temperatures from 20° C. to 280° C., and find that they indicate a curve, which slopes down gradually as the temperature is increased, while the deflections during cooling give a curve which is somewhat lower at the lower temperatures.

It appeared an important matter to ascertain whether the electrometer used in all these observations gives deflections strictly proportional to the inducing charge. To calibrate it by means of cells would have required a very large number; hence the following

method suggested by Professor Tait was adopted. A charge was put upon the inducing ball of the pair on the stand, and the deflection read; the charge was then divided by bringing an equal ball into contact with the inducing ball, the deflection read, and so on. The deflections were so nearly halved each time, that we may infer that they are strictly proportional to the charge on the inducing ball. We had arranged to verify our former observations in this matter last Thursday forenoon (27th June); but as the deflection on the scale always fell in the negative direction, and went to a great distance beyond the proper zero when the dividing ball was brought into contact, we gave it up. This effect was, doubtless, due to a strong negative electrification of the air; for the thunderstorm came on immediately.

2. On the Wave Forms of the Vowel Sounds produced by the Apparatus exhibited by Professor Crum Brown. By Professor Fleeming Jenkin, F.R.S., and J. A. Ewing, B.Sc.

At a recent meeting of the Society, Dr Crum Brown exhibited a gutta percha bottle of irregular form, which, when applied as a resonance cavity to reeds of various pitches, gave very good imitations of certain vowel sounds. By closing certain apertures in the side of the bottle it could be made to say A ("f*a*ther"), A ("*a*we"), O ("*o*h"), and I ("mach*i*ne"). When the cavity was kept constant, and the pitch of the reed was altered, the same vowel continued to be given. Dr Crum Brown was good enough to lend us the apparatus, in order that we might investigate the sounds given by the bottle in the same way as we have been investigating certain human vowel sounds, by obtaining and magnifying phonographic traces, and then subjecting them to harmonic analysis as far as the sixth partial tone. Of the vowels which the bottle speaks, O is the only one which we have fully examined in this way when spoken by the human voice, and we have confined our attention to it among the artificial vowels also.

By using reeds of various pitches we have obtained curves or traces of the artificial O's sufficiently good for harmonic analysis on the following pitches:—*e*, *f*♯, *g*, *b*, *c*′, *e*′♭, and *e*′. The pitch was in each case determined by measuring the length of the traces. The

vowel quality of the sounds, as repeated by the phonograph, was exceedingly good, even better that the original sound, as the jarring noise of the reed was lost. The sounds were thoroughly recognisable as O, of perhaps a somewhat bright species. The table below shows the amplitude of the successive partial tones, along with their absolute pitch to the nearest semitone.

Partial.	1 On *e*.		2 On *f*♯.		3 On *g*.		4 On *b*.		5 On *c*′.		6 On *e*♭′.		7 On *e*′.	
	Amplitude.	Pitch.	Amplitude.	Pitch.	Amplitude.	Pitch.	Amplitude.	Pitch.	Amplitude.	Pitch.	Amplitude.	Pitch.	Amplitude.	Pitch.
I.	3·8	*e*	4·3	*f*♯	9·8	*g*	9·1	*b*	4·2	*c*′	2·9	*e*♭′	2·1	*e*′
II.	7·3	*e*′	6·8	*f*♯′	9·0	*g*′	5·2	*b*′	4·5	*c*″	2·3	*e*♭″	3·1	*e*″
III.	3·9	*b*′	2·8	*c*♯″	4·8	*d*″	0·8	*f*♯″	0·5	*g*″	0·2	*b*♭″	0·1	*b*″
IV.	2·1	*e*″	1·0	*f*♯″	1·6	*g*″	0·3	*b*″	0·4	*c*‴	0·3	*e*♭‴	0·1	*e*‴
V.	0·6	*g*♯″	0·3	*b*♭″	0·8	*b*″	0·3	*e*♭‴	0·3	*e*‴	0·3	*g*‴	0·1	*g*♯‴
VI.	0·2	*b*″	0·2	*c*♯‴	0·1	*d*‴	0	*f*♯‴	0·2	*g*‴	0·1	*b*♭‴	0·2	*b*‴

It cannot be said that these figures show any specially strong resonance on or close to *b*♭′, which Helmholtz gives as the proper tone of O, but they do show a wide range of resonance, extending a long way above and below that pitch. There is distinct reinforcement as high as *g*″, or even *g*♯″, and as low as *e*′, if not lower, and partial tones falling anywhere between these limits are more or less reinforced.

The above analysis appears to show that a strong resonance on or near *b*♭′ is not essential to O, and that this vowel effect may be satisfactorily produced by other joint resonances above and below that pitch.

In a previous communication we pointed out that if the view be adopted that the constituents of the O's, sung at various pitches by a human voice, are due to the reinforcement caused by a constant oral cavity, the results of our analyses showed that this cavity not only has the property of strongly reinforcing tones close to *b*♭′, but must also be capable of strengthening, more or less, tones widely distant from that pitch, and extending over a large range. The analysis of the artificial O's now shows that a constant cavity may possess the latter property in quite a sufficient degree.

In order, however, to test this still further, we made the following experiment. A tube consisting of a piece of cane of the same size as one of the reeds was put into the neck of the bottle in place of the reed, and the side apertures of the bottle were closed so as to arrange the cavity for the vowel sound O. The end of the tube was then inserted in the ear, so that the whole apparatus acted as a resonator to sounds from outside. Then, striking the keys of a piano in succession, we observed what notes gave the peculiar humming effect due to reinforcement by the resonator. On working down the scale the resonance first became appreciable on $g\sharp''$. It then got stronger and stronger down to f'' and e'', which were both intensely and nearly equally strong. $e\flat''$ and d'' were a little weaker, but still very strong, and on $c\sharp''$ the resonance again became very intense. c'' was a little weaker, but also very strong. The resonance continued as the pitch fell, being sometimes stronger and sometimes weaker. g' and $f\sharp'$ were both strong,—decidedly stronger than could be accounted for by the reinforcement of their second partials g'' and $f\sharp''$. f' was weaker,—so much so that the resonance observed on it might be due to the second partial.

The presence of the upper partials in the notes struck made this method of testing the resonant qualities of the cavity inapplicable to pitches below those named. But the above cases, in which the reinforcement was distinctly of the prime, sufficed to show that the cavity would strengthen any tones between $g\sharp''$ and $f\sharp'$, at least, some more and some less strongly, while they left it an open question whether there was not resonance down to a lower limit of absolute pitch. Of course, it is to be observed that the bottle, when applied to the ear in the manner described, might differ in its resonant peculiarities from the same bottle applied to a reed, but its range is probably as great in the former case.

When the bottle was arranged for the vowel sound A, and tested in the same way, the resonance was perceptible as high as e''', and the highest maximum occurred on c'''. In this case also there was reinforcement over a range of at least an octave.

Traces of the wave forms of the artificial O's spoken of in this paper will be given along with those of other O's when the full account of our work is printed.

3. Notes on the Fungus Disease affecting Salmon. By A. B. Stirling, Assistant Conservator of the Anatomical Museum in the University of Edinburgh. Communicated by Professor Turner.

It is widely known that a destructive epidemic has this spring appeared among the salmon of the rivers Eden, Esk, and Nith. The mortality among the fish has been so great as to cause considerable alarm among proprietors, salmon commissioners, taxmen, anglers, and the general public.

The newspapers inform us that within three days the watchers have taken out of the Esk as many as 350 dead salmon. All who have examined the fish carefully, agree in referring the disease to the presence of a fungoid growth.

The other fish in those rivers, as the smolts, trout, eels, lampreys, minnows, pike, and flounders, are also said to be attacked in a similar way to the salmon, and fears are entertained that the disease may become thoroughly established in the district.

In these circumstances, I have thought it might be interesting to describe the condition of some of the fish which have come under my observation. In March last my friend Dr Philip Hair of Carlisle sent me the fin of a salmon which had been affected by the disease, and requested me to state if possible its nature. Unfortunately, the fin was in a putrid condition when it reached me, and as a result of the examination, I could only state to Dr Hair that the disease was probably a fungoid one. A few days later I received from Dr Hair a fine specimen of a trout, but it was not stated whether the fish was taken alive or picked up dead. It was, however, quite fresh, and the effects of the disease were painfully exhibited on the carcase. A hurried examination of this specimen enabled me to inform Dr Hair that the disease was due to what I had previously suspected, namely, a fungoid growth.

While examining this specimen, I observed entangled among the fronds of the fungus foreign matter of various kinds, namely, torulæ or yeast fungus, triple phosphates, fecula, human hairs, hairs of the cat and the mouse, also desmids, diatoms, shreds of dyed wool and cotton, with other fragments of matter unknown to me. Respecting the torulæ, I, in my letter to Dr Hair, asked if their presence could

be accounted for by bakeries or breweries in Carlisle, whose refuse might have got into the river.

My letter was published by Dr Hair in the *Carlisle Journal* of March 29th, and in the *Field* newspaper of March 30th, and as worded it might have been inferred that I regarded the presence of bakeries and breweries as the cause of the disease. This was of course not intended. On 12th April I received two salmon and a trout from J. Dunne, Esq., chief constable of Cumberland and Westmoreland, all of them in a diseased condition. Mr Dunne requested me to make an examination of those fish, and hoped, on public grounds, that I might be able to discover the true nature and cause of the disease.

As a result of my examination of those fish I sent a preliminary report to Mr Dunne. This report was forwarded to the Fishery Inspectors, and was considered of so much importance that it was published in the *Times* and many of the provincial and local newspapers. Sir Robert Christison had also very kindly supplied me with a number of specimens from the river Nith, all of them affected with this disease. An examination of these has confirmed me in the opinion expressed in the report above referred to. All these fish had the disease in an advanced stage, being more or less affected about the head, chin, branchiostegal rays, and fins in every instance. One salmon had rubbed the chin till the lower jaw had nearly separated at the symphysis, the skin was rubbed off the branchiostegal rays, and the rays broken; a trout had the upper left jaw bare of skin, the bone worn and hanging loosely attached to the cheek, the pectoral fin of the left side in rags, and the rays worn to stumps.

Another salmon had the skin rubbed off the nose and crown, and the matted fungus covered the bare parts; the dorsal fin was quite destroyed, the strong anterior rays being reduced to stumps of half an inch in length, and the remains of the fin bare, bleached, and without membrane. Beneath the dorsal fin on each side were spaces extending 3 inches forward towards the head, and 2½ inches backwards toward the tail, thickly covered with the fungus. Besides these there were other spaces on the sides of the fish from 1 inch to 2 inches in diameter, all covered by the fungus, which gave the fish a spotted appearance.

This fish appears to have been alive when taken, as the skull and brain had been punctured by the fisherman. The greater part of this fish was cooked; it was very firm and fat, and the three persons who made a meal of it pronounced it capital. I tasted a portion of the flesh from a part where the fungus covered the skin, and could not detect anything different in the flavour from an ordinary fishmonger's salmon.

The fungus appears, in the first instance, to attack those parts of the fish that are not covered with scales, as the crown, nose, sides of the head, chin, throat, and the membranous parts of the fins. From those parts the fungus extends by vegetative growth (which seems very vigorous), to those portions of the surface of the body which are covered with scales. On the sides of the fish where small patches of the fungus were situated on the scales (and no rubbing had taken place) no sore could be detected, and the fungus was easily wiped off with the finger.

I may also mention that all the fish which I received from the Eden river, both trout and salmon, were infested with tape-worms of a large size, the worms being about 2 yards in length and $\frac{3}{16}$ths of an inch in breadth. One of the salmon had from 60 to 80 yards of those worms in the pyloric portion of the gut. Another salmon had three varieties of worms in various parts of its alimentary canal. 1*st*. In the stomach were many round worms, about 4 inches in length, tapering to each end, and as thick as ordinary whip-cord in the thickest part of the body; many of those worms were entangled among the gill rays, it being their habit to crawl there when the fish dies, and from their presence in this situation they are called gill worms by the fishermen. 2*d*. A small spiral worm, which attaches itself, by burrowing in the outer walls of the intestine, in the fat and pyloric appendages. 3*d*. Tape-worms seated within the pylorus and intestine.

On 30th May, I received from Sir Robert Christison a large salmon from the Nith. This fish was believed to have been to the sea, after being attacked with fungus, and was captured on its return. The specimen was a female, and had the roe about one-fourth grown; the viscera were very healthy, and no entozoa were found in it. The head of this female is peculiar in having a kip on the under jaw, and a cavity in the upper jaw to receive it,

as in the male fish of the species. The right side of the head, including the eye and nose, was very deeply rubbed and the bones injured, but no fungus adhered to the injured part. The pectoral fin on the same side had no membrane, the rays being bare, broken, and separate from the muscles at their roots. There were several patches on both sides of the fish, from which the scales were rubbed off, but no fungus adhered to the rubbed parts. In several of those rubbed parts, although the skin was unbroken, a portion of the muscle, corresponding in breadth to the external injury, and half an inch in depth, was in a pulpy condition; beneath other rubbed spots the muscle was quite sound. The dorsal, ventral, caudal, and anal fins were all more or less injured by rubbing. No fungus adhered to any of the fins except the anal, the rays here being reduced to stumps of an inch or half an inch in length, on which a thickly matted covering of fungus is seated. The branchiostegal rays are very slightly rubbed, and are the only other part of the fish on which the fungus remains. In my report to the Fishery Commissioners in April last, I stated that the fish did not die of the fungus, but of the injuries they inflict by rubbing in trying to rid themselves of the pest. As some objection was taken in regard to this statement, I quote, in corroboration of my views, from a letter published in the *Field* of 25th May last. The letter was written by Commander Duncan Stewart, R.N. He says:—"In regard to the disease from which salmon are suffering in some of our rivers, it may be of advantage that I should mention what I observed in a small river at the head of Castrie's Bay in Siberia. I found the river rather low, but with plenty of clear running water. But what astonished me was to see thousands of salmon in all stages of disease and death, some darting away, but soon stopping to rub the side on the bottom or on a rock; others were constantly rubbing, others unable to rub. In those last cases large sores, from the size of a shilling to that of a half crown, of a most filthy appearance, were always present. Fish in which the scales had been rubbed off would try to get out of my way, but I could kill them with a stick; those with the skin gone would rub themselves against my trousers."

Supposing this salmon from the Nith had been to the sea, and had while there got rid of the greater part of the fungus with

which it was affected, it had returned to the river in such a mutilated condition, and with unhealed sores of such a nature, as in all likelihood would have ultimately proved fatal. Besides, the fact that the fungus was not killed by the salt water, but was found in a highly vigorous condition on the parts to which it still adhered, gives but small hope of any permanent benefit to diseased fish from a visit to the sea.

The fungus belongs to Saprolegnieæ, a natural order of doubtful affinity—said to have the habits of moulds and fructification algæ. This order consists of the genera saprolegnia and achlya, which are great enemies of fish and other animals preserved in aquaria.

The filaments of the fungus arise free from the outer surface of the epidermic layers of the fish, having neither branches nor articulations. They are tubes, the walls of which are perfectly translucent, and in their interior at irregular intervals are small groups of fine granular matter.

The majority of the filaments are spear-shaped at their upper terminations, and appear to be barren.

The prolific filaments, on the contrary, enlarge at their upper extremities, and form elongated club-shaped chambers, in which granular matter gathers. In the midst of this granular matter small round bodies appear, and those enlarging, gradually develop into spores. The prolific filaments apparently contain more granular matter, and are of greater calibre than the other filaments. They are evidently destined from the first to be the propagating media.

The spores escape by an opening in the summit of the chamber. This aperture is not an original opening. It is produced in a somewhat remarkable manner—so long as the spores are unripe and unfit for expulsion, a slender continuation of the filament projects from the apex of the chamber in manner similar to the neck of a bottle. At the point at which this joins the spore-sac, there is a slight contraction, which goes on gradually increasing in depth. Ultimately, when the spores are fully matured it drops off, and the aperture is formed. The filaments forming the mycelium of the plant are tortuous and branched; they ramify in the mucous and epidermic layers of the fish; they do not penetrate the corium where there are no scales. In other situations they never reach a greater

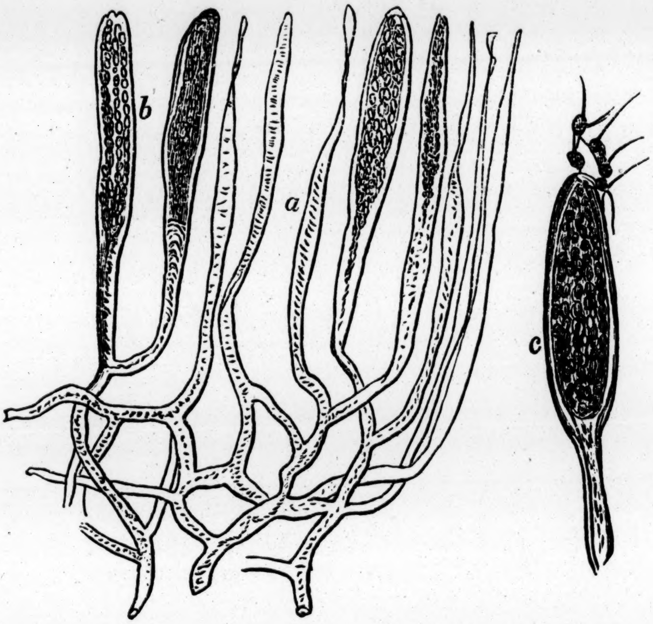
depth than the outer surface of the scales; they are tubular, the whole plant being without septa forms a single individual of apparently indefinite extent. The spores are variously shaped at different stages, ovate and kidney being the commonest forms. They are very minute, and require a power of 450 to observe them well. The cilia are two in number, a longer and a shorter one, and are situated at the long axis of the spore. They are difficult to observe, and always disappear in permanently mounted preparations, although the spores themselves remain unaltered in all other respects. When the fungus is stained with logwood or picric acid, excellent permanent preparations can be got. It has been stated that the fungus dies with the fish. I have not found this to be the case; on the contrary, all my observations have been made from dead fish. Some of the specimens sent me from Carlisle by Mr Dunne were mis-sent to Aberdeen, and returned to me on the seventh day after the death of the fish, and yet I have scores of permanent preparations from these specimens, which show distinctly the characteristic form of saprolegnia ferax.

I have also found the fungus perfectly identical in all the specimens I have examined, which consist of salmon, sea trout, and river trout from the Eden, and salmon and grayling from the Nith.

It has also been said that a salt solution destroys the fungus, "*which melts in the solution like sugar in water.*" On the contrary, salt and water is an excellent preservative of saprolegnia; masses of it before me as I write have been in a salt solution for two months, and it remains unaltered. Further, the salmon captured in the Nith, which is believed to have gone to the sea in order to get rid of the fungus, had the fungus growing vigorously on several parts of its body. The fungus must either have instantly attacked the fish on its return to the river, or not have been destroyed during its stay in the salt water.

Regarding the cause of the disease, I can offer no opinion further than that some functional condition of the fish seems necessary for the propagation of the fungus. The germs of saprolegnia ferax must exist at all times, and in many places; and if so, there must be a reason why fish are not constantly affected with the fungus and in every river. I am persuaded that the condition of the fish

is in some way either suitable or unsuitable for the propagation and growth of the fungus. Whether this arises from too high or too low condition, I am quite unable to say; but I may remark that while some of the fish examined were in the kelt stage, others were in a condition perfectly fit for food.



Saprolegnia ferax parasitic on the Salmon.
a. Barren filament. *b.* Prolific filament. *c.* Prolific filament, more highly magnified.

4. On some New Bases of the Leucoline Series, Part I. By G. Carr Robinson.

5. On the Crystallisation of Isomorphous Salts. By G. Carr Robinson.

It is generally stated that isomorphous salts are capable of crystallizing together in any proportions, or that the isomorphous elements which enter into them are capable of replacing one another in any proportion; *e.g.*, potash alumina alum and potash iron alum can crystallise together in all proportions.

Hauer * states that mixed crystals of alumina and chrome alum grow in a solution of ammonia iron alum; and, again, he states † that the more soluble isomorphous salt completely hinders the solution of the less soluble, so that if solutions of common alum, chrome alum, and iron alum be mixed, precipitation of the less soluble alums will occur.

The present paper is the result of some experiments made with the four alums—potash alumina, ammonia alumina, potash chrome, and ammonia iron.

The solutions of the two first, potash alumina and ammonia alumina, were obtained by saturating water at 100° C. with the salts, and pouring off the mother-liquor from the crystals that deposited on cooling.

The chrome alum solution was obtained by saturating water with the alum at a temperature carefully kept below that at which the green modification of chrome alum is produced.

Whilst the solution of iron alum was made by saturating water, acidulated with sulphuric acid, with the alum, at about 50° C.

The following experiments were then made and observed:—

a. When a crystal of potash chrome alum is placed in a solution of potash alumina alum exposed to the air, the chromium is turned out by the alumina, the interior of the crystal becomes granular, whilst a clear shell of alumina alum grows over it, the faces of which are finely striated.

b. When potash chrome alum meal is added to a solution of potash alumina alum in a well-closed bottle, and kept at very nearly a constant temperature, the chrome alum dissolves, but no replacement takes place.

c. When a crystal of potash chrome alum is placed in a solution of ammonia alumina alum exposed to the air, the ammonia alumina alum grows on it, there being only very slight replacement.

d. Potash chrome alum meal added to solution of ammonia alumina alum in well-closed bottle, and kept at very nearly a constant temperature, no change takes place.

e. Ammonia iron alum grows on crystals of potash alumina alum.

f. Ammonia iron alum grows on crystals of potash chrome alum.

* "Sitzungsberichte," Imperial Academy of Sciences, Vienna, 1860, xxxix.
† "Sitzungsberichte," Imperial Academy of Sciences, Vienna, 1866, liii.

g. When a crystal of ammonia iron alum is placed in a solution of ammonia alumina alum, replacement goes on very slowly, rust at the same time being thrown down.

h. Potash chrome alum grows on crystals of potash alumina alum.

i. If a crystal of ammonia iron alum be placed in a solution of potash chrome alum, the iron alum is turned out, whilst a skeleton in chrome alum of the original crystal is left; if this skeleton be placed in a solution of ammonia alumina alum, the latter alum grows on it, completing the form of the original crystal of ammonia iron alum.

6. On a New Method for the Separation of Yttrium and Erbium from Cerium, Lanthanum, and Didymium. Part I. By J. Gibson, Ph.D., and R. M. Morrison, D.Sc.

The method for the separation of these two groups hitherto in use was first proposed by Berzelius, and has been followed by almost all chemists who have investigated these earths. For the details of this method we must refer to his "Handbuch;" but we may briefly state that it depends on the relative solubilities of the double sulphates of these metals with potassium in a saturated solution of potassic sulphate. The yttrium and erbium double sulphates are said to be easily and completely soluble, while the double sulphates of cerium, lanthanum, and didymium are said to be perfectly insoluble. Wishing to prepare pure salts of yttrium and erbium, in order if possible to obtain the metals and to determine their specific heats, we tried this method, and found that, although it is a good rough method, the separation is by no means complete. We repeated the separation six times, but never obtained the earths pure, the spectroscope always showing the characteristic absorption-spectrum of didymium, provided we examined a sufficiently thick layer of a saturated nitric acid solution. The incompleteness of this method is indeed acknowledged by Bahr and Bunsen in their well-known paper on these metals.* The test given for the presence or absence of these two groups by these chemists was the presence or absence of the absorption-spectra of didymium and erbium respectively. We found, however, that

* Ann. d. Chem. u. Phar. cxxxvii.

not only are the double sulphates of the didymium group somewhat soluble in a saturated solution of potassic sulphate, but that some erbium, if not also some yttrium, is precipitated. Repetition of this process fails to remove the last traces of the didymium group.

After trying various modifications of this method, some of which gave better results, notably boiling the double sulphates with the saturated potassic-sulphate solution and filtering hot, we determined to look for another method, none of these variations being sufficiently good.

We have obtained better results by the following method. After having extracted as much as possible of the didymium group by the old method, the earths are dissolved in nitric acid, and to this solution a large excess of a solution of carbonate of ammonia is added, and the whole allowed to digest for a day in a closed flask, the precipitate being frequently shaken up. The liquid is then filtered and the undissolved residue washed. On acidifying the filtrate with hydrochloric acid, and adding oxalic acid, a precipitate is obtained which, on ignition, yields the oxides of yttrium and erbium, free from, or only containing the merest trace of, lanthanum or didymium, which may be removed by a repetition of the process. Any cerium originally present goes into solution, and must be removed by boiling a solution of the sulphates with carbonate of magnesia. The undissolved residue of the carbonate of ammonia solution still contains yttrium and erbium carbonates, but is much richer in didymium, and must be treated with a fresh portion of carbonate of ammonia solution.

It is essential that the ammonium carbonate solution be neither too concentrated or too dilute, as in the first case some lanthanum and didymium dissolves, and in the latter the carbonates of yttrium and erbium, after dissolving, crystallize out as double salts, leaving almost nothing in solution. The strength we found most suitable was about half saturated.

7. On certain Effects of Periodic Variation of Intensity of a Musical Note. By Professor Crum Brown and Professor Tait.

Recent discussions as to the nature of vowel-sounds have led us to make experiments (partly with an apparatus constructed some years ago for the same purpose) upon the effect of a periodic variation of intensity of a simple tone.

It is obviously impossible to secure a simple harmonic variation, so we endeavoured to produce a displacement varying as

$$1 - \cos mt.$$

An organ-pipe, giving a tone very free from harmonics, was sounded on one side of a partition in which were cut a series of large holes. These were opened and shut periodically by a revolving disc cut into separate sectors. The form of the holes was calculated on the rough assumption that the intensity of the sound passing through them was at each instant proportional to the uncovered area of the openings. This may be approximated to in many ways, most simply by making the holes approximately square or of rhombus form, with one diagonal radial, and the corners at the ends of that diagonal somewhat rounded off.

Supposing the adjustment perfect, the result should have been the disturbance

$$(1 - \cos mt) \cos nt,$$

or

$$\cos nt - \tfrac{1}{2} \cos (m+n)t - \tfrac{1}{2} \cos(m-n)t.$$

Thus, in addition to the tone given by the pipe, there should be two others of the order of summation and difference tones. The result was tested very easily by the help of resonators. Standing in front of the openings in the partition, the observer applied a resonator to each ear, and the pipe giving the tone whose number of vibrations was the arithmetical mean of those of the tones of the resonators was sounded on a small organ, and the disc made to rotate with gradually increasing velocity. The resonators were found to be affected simultaneously.

The experiments, as we made them, succeed much better with

low notes than with high—because, though the rotating apparatus always produces a siren effect, the intensity of this effect is very small at low speeds. A very curious case occurs when $m=n$ (*i.e.*, the siren giving the same note as the pipe), for then the first harmonic comes in very marked.

8. Note on a Mode of Producing Sounds of very great intensity. By Professor Tait.

Two years ago I had an opportunity of making from the deck of the steamer "Pharos" some observations on the performance of the fog-siren at Sanda, off the Mull of Cantire. The instrument is worked by air at about $1\frac{1}{4}$ atmospheres pressure; and, though driven by a powerful air-engine, sounds for 7 seconds only per minute. One obvious defect of such an arrangement I saw to be the waste of energy in producing a current of air through the trumpet of the siren along with the oscillations. It then occurred to me that a regular alternation of puffing and sucking—exactly analogous to the air-disturbance produced by a drum—must be a much less costly source of sound. I have since constructed a siren on this double action principle, the air in the trumpet, which acts as a resonator, being put alternately in connection with reservoirs of compressed and rarefied air. The small model has given very good results, and a larger one is in progress. The only defect which my model showed was a waste of energy in the form of pulsations in the tubes leading to the exhausted receiver and to that containing compressed air. This can be very greatly reduced, but I do not yet see how to get rid of it entirely, unless it be possible to make both receivers so exactly as to act as additional resonators to the siren. If this can be carried out in practice there will be no energy spent except in sound. It is obvious that the principle just described is approximated to in practice whenever steam is employed in a siren:—the vacuum being produced by the condensation of the steam.

Another device of a somewhat different character was suggested to me by the experiments described in the preceding paper. After trying, without much success, to reduce the intensity of the siren notes by filing the edges of the apertures, it occurred to me that I

might usefully *intensify* them. I therefore had copper plates soldered perpendicularly to the revolving disc, so as to increase instead of diminishing the virtual thickness of the edges of the apertures. The result was very striking. Such a siren gives a sound whose intensity is not sensibly increased by a powerful blast from an organ bellows. It produces strong currents of air through the holes in the fixed disc, whose direction in general depends upon the direction in which the rotating disc is made to revolve; and especially does so when the copper plates are inclined to the surface of that disc. When the discs are both furnished with these plates, turned in opposite directions, the result is still more striking. Various other modifications have occurred to me, and are now under trial, especially one for producing currents alternately in opposite directions through the holes.

By bringing up a flat plate towards the instrument, the quality of the sound is altered in a remarkable manner, and to such an extent that it seems well adapted for rapid Morse-signalling. As this instrument requires no work to be spent except in turning it, a very large number may be kept continuously at work at once by the same expenditure of power as is required for the intermittent roaring of a single fog-siren.

The following Gentlemen were duly elected Fellows of the Society:—

James R. Stewart, M.A. Oxon., 10 Minto Street.
John Archibald Campbell, M.D., Garland's Asylum, Carlisle.

Donations to the Library of the Royal Society during Session 1877–78.

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SESSION 1877–78.

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